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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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The Journal *of* Parapsychology

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EDITORIAL

PARAPSYCHOLOGY AND PHYSICS

As PARAPSYCHOLOGY is defined today the question of its relation to physics is of the first importance. Indeed, it takes its present significance largely from the relation its phenomena are considered to have to the physical universe; accordingly, the present discussion should begin with definitions.

Parapsychology has only in relatively recent times been defined in general terms. It went for a comparatively long time identified only by its problems. These were called psychic, metapsychic, or parapsychological, but what such terms meant was not generally defined during the first fifty years of the research. When, as late as the thirties, attempts were made to distinguish and characterize this problem area, the criteria suggested were that the problems of parapsychology all related to phenomena attributable to personal agency and that they challenged the recognized principles of scientific explanation in the natural sciences.

Only within the last few years has a sharper line of definition been suggested. It has recently been submitted that what more specifically identifies all the phenomena called parapsychological is that they appear to defy *physical* explanation. Parapsychology, then, deals with those manifestations of personality that are regarded as nonphysical in nature.

This definition should not be regarded as a merely negative one. Quite obviously the definition of parapsychology does depend upon

the distinction of its area from that of physics; but every other definition, if it is clearly enough outlined, necessarily has similar negative delimitations. Parapsychology derives its more *positive* character from the fact that its processes belong to the area of personality. They are sectioned off from the rest of the field of psychology by the apparently nonphysical character of the phenomena.

Nor should this definition be construed to imply that all the rest of the personality does lend itself to physical explanation. Rather, the definition merely draws a line around those occurrences that seem beyond interpretation by the kind of principles involved in the sensorimotor relation of the individual to his environment. The rest of the phenomena of personality are shrouded in ambiguity. They may or may not represent nonphysical processes at work. General psychology of the conventional type—that is, excluding parapsychology—has not gone far toward discovering whether or not its more familiar reactions belong entirely within the domain of physics. Metaphysical prejudice against anything suggesting dualism still keeps many psychologists from looking at anything that might be nonphysical.

But while psychology has been assiduously trying to erase the line distinguishing it from physics, parapsychology has been drawing one that is unobliterable by any of the intellectual devices thus far applied. If the progress of parapsychology has meant anything, it is that. And if the original spontaneous psi occurrences which inspired the first experimental work only *suggested* a human function that was nonphysical in character, the psi experiments have brought the issue into much sharper focus. Taken as a whole, the psi tests involving various distances, barriers, and other physical conditions, together with the results from the experiments in precognition, lead to the conclusion that the processes at work show no regular relation to space and time in the researches concerned. In the absence of any systematic relation of the results of any of the psi experiments to the familiar criteria of time, space, and mass the evidence from the experiments thus far conducted indicates *that we are not dealing with a physical process.*

Parapsychology admittedly has not gone as far in any of its

branches of research as is desirable. Every one may come to his own decision, however, as to how unreservedly he wishes to draw conclusions regarding the establishment of the nonphysical boundary of the psi field. Until one is sure of this boundary, of course, he can have no great confidence in parapsychology's distinctive claim. But for those who are led by the evidence to recognize the extra-physical nature of the psi functions in man, parapsychology may be said to have a proper and legitimate charter to that domain of the science of personality that transcends the conditioning of space, time, and mass.

* * * * *

We come now to the question of just what physics is. Like every other branch of science, it has been expanding rapidly, and with each major advance the conceptual boundaries of the field are likely to be altered to some extent. There is some disagreement and uncertainty among physicists as to just how much of current physical theory is sufficiently settled for acceptance, and the task of drawing a boundary line that would satisfy everyone is a difficult one. Fortunately, we need not attempt to arrive at an up-to-the-minute definition for our present purposes—that is, to secure sufficient agreement on whether or not the phenomena of parapsychology are nonphysical.

Whatever uncertainty may prevail on the frontiers of current physical theory, there is thus far no abandonment of any of the criteria of time-space-mass relations that are conceivably involved in the simple physical relations present in psi experiments. No reason has ever yet been suggested why this set of criteria cannot be followed with reliance so long as they continue to be fundamental to the description of the physical operations of the universe.

As in every field of research, we must be prepared to add or substitute better discriminatory criteria and better working concepts as we find them, and, of course, to examine afresh all the questions involved. No one can say that the present conception of psi phenomena and the boundary of parapsychology are permanent; such judgments cannot be passed even in older fields than ours. It will admittedly be a vast research program, and a necessary one, that will determine the full *extent* of the operations of psi phenomena in

their defiance of time-space-mass limitations and bring more sharply and clearly into view the true nature of the boundary. In the meantime, such a program should not be discouraged out of mere linguistic differences over definitions of fields of research as fluid and unpredictable as those of parapsychology and physics both appear to be.

* * * * *

The real issue, however, is not merely one of whether or not psi phenomena can be explained by *physical* principles. It is rather a question of whether man is a relatively simple, one-system creature or whether he has the much greater complexity attaching to a combination of two different systems of lawful operations. A backward glance over history reminds us that the great philosophic issue of materialism has been challenging western culture and its institutions more and more seriously for several hundred years. The growing influence of the physical sciences has colored the philosophy of western thought and has forced upon biology and psychology an increasingly thoroughgoing physicalism, making physical science practically synonymous with nature. Psychology in Europe and America, following physiology and physics, has tried devotedly to describe and explain its phenomena in physical terms and principles. We cannot be entirely certain, we can only suggest, that its manifest failure to account for human nature is largely due to the restriction of this physical pattern upon psychological thought and observation.

It can be said, however, that in the psi researches psychology has now had demonstrated for it, by procedures it has not been able to disqualify, that it has a principle of explanation not tied to the physics of time-space-mass. How far the mere beginnings on psi functions can lead into the great unknown areas of human personality and the mysteries of its place in nature no one can say, but at least the gates have been opened for a vast new area to be explored. If conscious purpose, intelligence, emotion, volition, reason and the like, so basic to the whole meaning of human life, have taken on no significant advance in understanding whatever from the enormous program of mechanistic science, we may well invite psychology to try, timorously if it must, the consideration of a more compound

structure of personality, one in which functions have a place that defy the laws of matter and operate in ways that are foreign to the concept of the machine.

The kind of absolute dualism that drove psychology into the arms of physics is concededly not defensible. We cannot logically conceive of two completely different systems interacting and composing so manifestly unified a whole as the personality of man without having something in common. But this is not to justify the rejection of *every* differentiation within the whole; fears of absolute pluralism should not blind the scholar to distinctions that are matters of experimental observation. We can perfectly well see new diversifications, new properties, or new stratifications to personality without going to the extravagances of the philosophy of a seventeenth-century dualism. Rather, observations should lead back from the phenomena to the underlying causal nexus in some deeper-lying substratum where the basis of unification can be discovered. Thus faith in the integrity of the universe may be restored for those who are worried about it.

But let us contemplate the extreme contingency. If ever the shifting labels of the future do come to include in the coverage of physics an order of causation independent of space-time-mass relations and capable of motivated, intelligent action, even so, the only change in the issue for personality will be in the physics label itself. The problem of the nature of man will still be there. What is really significant with regard to that great question is the fact that there is a part, a process-system, in man with properties and principles of its own, one that is more capable of explaining the subjective life of the individual than are the properties and principles associated with matter. That is no mere trifle of wording.

* * * * *

For the proper accommodation of psi phenomena there is no doubt that the concept of reality must be extended. There is, however, no good reason for supposing that reality is limited to processes affected by time, space, and mass. It is well known that both philosophers and theoretical physicists have assumed various kinds of substrata of reality underlying the experience of physical phenomena. These are generally assumed to be beyond the present reach of observa-

tion and measurement, certainly beyond the sensory range of man—as for that matter most of what concerns modern physics likewise is.

According to our ordinary logic of causation, the establishment of psi phenomena and their properties calls for the supposition of a special determinant capable of producing (directly or indirectly) the results measured in psi experiments—another "energy" we would say if the word, defined as "the capacity to do work," is not too closely limited to physical significance. Such a psi determinant should be supposed to be convertible to one or another of the known energies since only thus could its operation be discovered and its effects measured.

Back of psi and all the other phenomena of nature we need to infer the presence of a common energetic reality. There must be such a source of the known physical energies, some of which affect the sense organs and which produce phenomena more or less limited by time-space-mass relations. Such a common store must be at the same time the source of this psychic energy that does *not* affect the senses, does *not* produce effects directly related to time, space, and mass within the range of conditions investigated, and yet *does* produce results, results that are indirectly observable when converted into the forms of subjective or objective record that can be perceived.

We cannot, of course, seriously suppose that as the psi explorations continue *no* limitations will be found in their relation to time, space, and mass. Neither, for that matter, is it known to what extent *physical* operations are exempt from one or another of the usual time-space-mass limitations generally applying to physical process. Certain physical phenomena are described as presenting exceptions to the usual laws of operation governing physical bodies. Under certain conditions an object may have no mass, under another a phenomenon may be timeless, and under certain conditions a function is spaceless. There is to our knowledge, however, nothing but a psi phenomenon that appears to defy all these criteria of physical operation and at the same time displays intelligent purpose in the process.

Since physics, as well as parapsychology, must find its causal roots, and since its boundaries and criteria are more fluid than those

of most fields of science, we may expect help from it in the search for the common ground of energy interchange which enables psychical and physical interoperations to take place within the personality. It may profit physics, as well as parapsychology, to push the precognition research further still into the more difficult ranges of timing, for time is still an unknown in physics as well as in psychology. Here, too, we shall touch from a fresh angle the root problem of man's volitional freedom from the physical principles of his body and environment—perhaps a key problem of human life today.

For those frontier areas of physics, biophysics, and psychophysics, the establishment of a psi energy should open a new prospect and widen the range of problems. Hanging over the entire world of life and growth there is still as much of a mystery as there is in the world of the mind as to just what the underlying organizing factors and directive forces of the living organism really are. The full sweep of this puzzle problem for biology is pictured by Dr. Sinnott in *Cell and Psyche,* wherein he points out that the attempt to find a *physical* explanation has not been successful. Some biologists have maintained that life and growth, in the creation of the new organism to match an inherited pattern, suggest the operation of factors nonphysical in character. No one, however, has found a way as yet to bring the hypothesis to an adequate experimental test.

Now that nonphysical operations have been established as occurring in psi phenomena, however, biology is given its liberation from materialism. And if Dr. Sinnott is correct in his hypothesis that the elaborate mental system of man is a continuation of the organizational factors operating in the simplest cell, the way may be opened to a wider biological significance for psi phenomena than would otherwise have been reasonable.

It may be significant to remember that the operations producing psi phenomena are unconscious to the subject in whom they occur, as well as nonsensory and nonphysical. There is much else to suggest that they derive from a primitive biological system. They may represent a relation to the physical environment on some simpler protoplasmic level that permits operations which would elude the

sensorimotor system of the organism, operations that utilize energies that physics could only intercept through their converted effects. It is indeed significant, if correct, that all *conscious* experience appears to depend on that limited range of natural operations that are perceptible by the sense organs, experienced either through direct sense perception or a reactivation of it as in imagery and recall. Should it be, as it appears, that consciousness is tied to physical (time-space-mass conditioned) energetics for its basic stimulation, and that the unconscious psi-system is based upon as yet undiscovered nonphysical processes in nature, we shall have a first faint glimpse into one of the most central of the mysteries of life and man, necessarily speculative though the glimpse must be. It is tempting to suggest that what physics has done for sensory physiology and psychology, which is a brilliant chapter in science, the study of the role of psi energetics might do for the immense range of problems in biology that have so long defied physical explanation.

In any event, however right such tentative reflections may be, the findings of parapsychology can be of help in freeing the life sciences from the bondage imposed on them by physicalistic ways of thinking. In so doing they may be expected to introduce a broader concept of causation, operating on some deeper plane of interaction to relate the basic processes of life and personality and matter, of sense and psi, of organization and volition. Thus they may guide the way toward a truer and more understandable concept of man's place in nature.

J. B. R.

A REPORT ON AN EXPERIMENT IN PSYCHOKINESIS WITH DICE, AND A DISCUSSION ON PSYCHOLOGICAL FACTORS FAVORING SUCCESS[1]

By Robert H. Thouless

[The following section is a condensed version of those parts of the original article which dealt with procedure and experimental results. They have been reduced here by the editors so as to provide only the details essential for an understanding of the section on psychological factors, which, except for a few minor adjustments, has been reproduced in full.—Ed.]

Procedure and Experimental Results

The following series of PK tests, in which the experimenter used himself as subject, were carried out during 1948-49. In these experiments the subject attempted to influence the fall of four dice to make them come to rest with a specified target face uppermost. He alternated target faces in such a way that each of the six denominations was the target for the same number of throws and the target faces[2] were distributed equally with regard to the order of throwing within each session. The investigation included four series, the conditions and general results of each being as follows:

Experiment I. In this series some of the dice were cup-thrown, and some were mechanically thrown according to the condition described below in Experiment IIA. A total of 16, 232 die-throws yielded a positive deviation of 103⅔ hits, with a CR of 2.18 (P=.03).

Experiment IIA. This was based upon machine-thrown dice. The procedure involved placing the four dice, marked *A, B, C,* and *D,* upon a hinged platform in the same order and position and with the same orientation of faces for each trial. The experimenter

[1] The original article appeared in the *Proceedings of the Society for Psychical Research,* Vol. 49, Feb., 1951, and is used here with the permission of the author and the Society.

[2] Dr. Thouless used Latin squares as a basis for choosing the PK targets in all but Experiment IIB. This method rules out precognition as a possible explanation of the results. The full description of the method and of its advantages is given in the author's original article.

then pulled a string to release the catch under the platform, allowing the dice to fall by gravity. They rolled down an incline covered with corrugated rubber onto the floor. This was the procedure for all machine-thrown series. Series IIA consisted of 2,592 die-falls, which gave a positive deviation of 42 hits, with a CR of 2.22 (P=.025).

Experiment I$^{\kappa}$. This experiment consisted of cup-thrown tests. It was carried out concurrently with Experiment IIA (after scoring in Experiment I had declined to the chance level) to see if this procedure would continue to give poor results while IIA was giving more successful results. A total of 1,728 trials gave 279 hits, which is nine fewer than mean chance expectation.

Experiment IIB. This experiment was called the $\Psi\gamma^{\kappa}$ series, as it involved both $\Psi\gamma$ (ESP) and Ψ^{κ} (PK). A set of six cards numbered one to six was shuffled and placed face-down on the table. The experimenter then threw the four dice (mechanically) six times, willing them on the first throw to fall so that the face would correspond with the number on the top card, on the second throw to correspond with the second card, and so on. The target faces were not known until all six throws were completed. Three sets of six throws were made on each experimental occasion. The total of 2,592 die-throws yielded a positive deviation of 33 hits, with a CR of 1.80 (P=.07).

Combined Results

All of the experiments combined consisted of 23,144 die-throws with a total score of 4,027 hits. This is a positive deviation of 169⅓ hits from mean chance expectation, with a CR of 2.99 (P=.003). Thus the work as a whole is statistically significant in terms of the total score.

Two kinds of decline effect which were observed in the data contributed evidence for the operation of PK that was independent of the total scores. One of these was a chronological decline in rate of success during the course of the experimental series. This type of decline was found in Experiment I, where the first 24 runs gave a deviation of +15 and the last 24 runs, −19, with a CR of the difference equal to 2.7 (P=.007). The other decline effect, a drop

in scoring within the experimental session, was particularly striking in Experiment IIB. Each session, it will be recalled, consisted of three sets of six throws each, one for each target face. When the data for all sessions were pooled it was found that the first set of six throws "around the die" gave a positive deviation of 38 hits (CR=3.68; P=.0003), while the second and third sets of the session each gave one hit less than mean chance expectation.

Favorable and Unfavorable Conditions for PK Experimentation

One of the objects of this series of experiments was to see whether I could find out anything about conditions which determined success or failure in PK experimentation. I was looking out for any hints that my experiments might give me, and the tentative conclusions I wish to report are often based on very incomplete evidence. I shall indicate this by using phrases which indicate incomplete conviction; where the evidence is reasonably good, that fact will be mentioned, but I wish to make it clear that I am not confining myself to reporting conclusions that I regard as established. Rather they are more or less tentative conclusions which must be tested out by later research. It was my impression that PK success was more affected by unfavorable conditions than is ESP success, that inhibition is more easily set up, and that success can only be expected if a good deal more attention is paid to the problem of securing optimal conditions than is necessary when experimenting with the more stable ESP processes.

The problems of conditions may be discussed under the following heads:

Motivation
Time of day
Chronological decline
Total length of experiment
Length of each experimental occasion
Induction of favorable psychological conditions

Motivation

It has always been my impression that in both ESP and PK

experiments on myself, effort to achieve the required result defeats its own end and tends to produce failure. Too strong motivation tends to result in effort and failure results. That some degree of motivation must be present seems likely but it must remain at a low level. Strong anxiety to succeed seems to militate against success. I have no convincing proof of this, but it is an impression gained by many indications. Effortless intention to succeed seems to me to be the ideal attitude. The way in which I have sometimes expressed this is that the attitude must be that expressed by the words: "I want to succeed but I don't really care whether I do or not."

A playful attitude towards the experiments is favorable to this relaxed motivation. At the end of December, 1948, I started doing PK experiments in competition with my son and other members of my family. The results of these competitions (for myself alone) are included in Experiment I. The rate of scoring was considerably higher than I was at that time getting when working alone. It might be supposed that making PK into a competitive game would intensify motivation rather than reduce it. For a child it might do so, but for an adult the desire to win in a game is a far less strong motive than to get a positive result in a scientific experiment. It is true that the latter motive was still present but it was largely suppressed by the more trivial motive of winning the game. Under these conditions I felt that effort was relaxed and I succeeded better. But a crisis in the game inducing effort could again reduce the score. For example, in my first game with my son in which we threw in turn a set of three runs (72 falls) equally divided between the six targets, we both obtained a score of 16 above mean chance expectation at the end of 9 turns (27 runs). This is a pretty good rate of scoring for a PK experiment. Then we decided to settle the game by throwing until one was two ahead. This immediately produced a state of increased volitional tension and, for the next turn, I scored three below expectation while he scored two below chance expectation (the first time we had both scored negatively).

The same result appears to be produced by anxiety as to the total result. On one occasion I made alone a series of 27 runs with an

equal number on each target face without paying attention to my total score. Then I added up my score and found it was 17 above mean chance expectation. Then I felt that this was fine and I must keep it up; in other words volitional tension was increased. I immediately made another series of 27 runs and scored 7 below mean chance expectation.

These are mere indications of no great evidential value but they are examples of the kind of observation which makes me inclined to think that relaxation of volitional tension is necessary to success. They suggest that it may be worth while for experimenters who wish to get success to try to make their attitude towards their experiments rather playful than over-earnest. Competitive scoring may be a good way to achieve this end. There is nothing original about this suggestion; the value of a playful attitude has been suggested by both Professor Rhine and Dr. Humphrey. It also suggests that it is well to avoid being anxious about the result. The best way of experimenting may be to predetermine the total length of an experiment and then not to work out total results until the experiment is completed. It is hard to deny oneself the satisfaction of seeing how the experiment is going on, and I do not observe this principle myself, although I believe it is the best way. It is also to be considered that self-experimentation is particularly unfavorable to a care-free attitude towards experimental results, since a subject who is also the experimenter cannot fail to be anxious about the total result. On the other hand, this may not be important since it is possible that anxiety and volitional stress on the part of the experimenter may interfere with successful results however care-free the subject may be.

Time of Day

On three days during the time when I was doing both experiments IIA and IIB, I did the experiment both in the morning and the late afternoon of the same day. The combined results for the two experiments show a startling superiority for the morning sessions. In every one of the six comparisons, the morning session showed a higher score and the total result was that given in the following table showing a clearly significant superiority for the total morning score.

	Falls	*Hits*	*Dev.*	*SE*	*CR*	*P*
Morning	432	95	+23	7.74	2.98	.003
Afternoon	432	66	− 6	7.74	.78	.44
Difference		29	29	10.95	2.65	.008

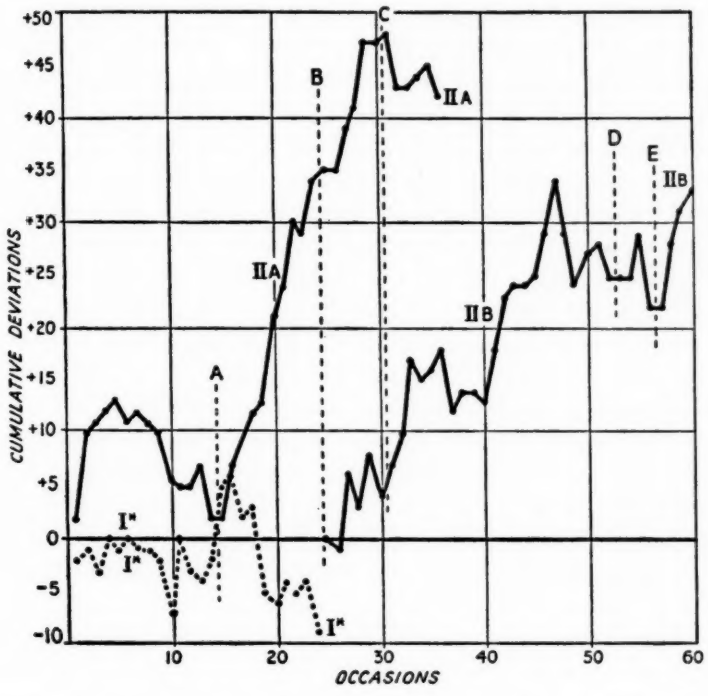
I was inclined at first to attribute this difference to having done the experiment twice in the same day, but it is more likely that it is due to the difference in the time of day, since it is found in a later experiment (not yet completed) in which afternoon experiments were done on different days from morning ones. In this experiment too there is a superiority of the results of morning experiments over afternoon experiments although those done in the evening are also better than afternoon ones.

The suggestion that experiments should preferably be done in the morning may seem a not very practical piece of advice for those whose occupation takes them away from home early in the day. For most people, however, it would be possible to devote half an hour to experimenting on Sunday mornings and it must be remembered that a short series of experiments under optimal conditions may give a better result than a long series under less favorable conditions with less total expenditure of time.

On the other hand, if morning is found to be too difficult a time for experimenting, evidence from later experiments than those reported here seems to indicate that evening is also a good time. The period of the day that seems most definitely unsatisfactory for positive results is the afternoon.

Chronological Decline

In the course of a PK experiment (as in an ESP experiment) a process of inhibition sets in which reduces scoring rate and which apparently may go on to a point at which the subject is scoring significantly below chance expectation. An experiment may, of course, be designed in which this process of decline is itself the object of study. To an investigator wishing to obtain a positive deviation, however, decline is merely a nuisance and much of the detail of experimental design (spacing of experiments, length of experimental occasions, etc.) is directed by an attempt to minimize this decline.



CUMULATIVE TOTALS OF EXPERIMENTS IIA, IIB AND I^x AT END OF EACH EXPERIMENTAL OCCASION

A. Point of beginning of practice of reciting poetry during the experiments.
B—C.—Between these points experiments were carried out alternately in the morning and the late afternoon with resulting alternate high and low scoring in both experiments.
D—E.—Between these points only one run (24 falls) was done on each occasion. In order to keep this part of the diagram uniform with the rest each entry refers to the combined total of three runs although these were done on separate occasions.

On each occasion, the total score obtained up to and including that occasion is plotted. The height of the accompanying graph shows, therefore, the cumulative total for that experiment, while the difference between two successive plots shows the score (deviation from chance expectation) on that occasion. I^x was an experiment with hand-thrown dice. I had been doing a good deal of this experiment and it was already thoroughly inhibited; nothing but chance results were obtained. Experiment IIA, started at the same

time, was in a form I had not been recently using. Target determination was the same as in I^x but the dice were machine-thrown. Positive scoring started at once but chronological decline appears to have set in on about the fifth occasion and the scoring continued predominantly negative until a novel feature (the recitation of poetry) was introduced at the point marked A. Thereafter, scoring was heavily positive until the thirty-first occasion when there are again indications of the onset of chronological decline, although the experiment came to a predetermined end (at the thirty-sixth occasion) too soon after this for any certainty that this decline was final. IIB was also a novel form of experiment started when I^x came to an end. The $\Psi\gamma^x$ experiment had been done previously with hand-thrown dice but not (as here) with machine-thrown dice. Again it started off with positive scoring which was maintained until the twenty-third occasion of this experiment. After this point, nothing was added to the score although there is some indication of an end spurt. This, however, may be merely a temporary consequence of having made an unfavorable modification of the experiment between the points D and E.

These observations suggest that at least some part of the decline effect may not be general, affecting all types of PK performance, but specific to the particular task whose repetition has produced the inhibition. If this is the case, an improved score may be expected if the subject is switched to another task. I found other indications of this specificity of the decline effect. For example, on January 3, 1949, during the course of Experiment I, I did six runs of a help-hinder experiment with a score of three below mean chance expectation. This was a familiar experiment. On the same day I started an entirely new experiment, the $\Psi\gamma^x$ form of experiment, and scored a positive deviation of +8 for 15 runs. On the following day, I continued at about this rate of scoring for the $\Psi\gamma^x$ but was still below mean chance expectation for the help-hinder experiment. There seems here to be a strong indication that an inhibition of success resulting from continued application to one kind of experiment may not inhibit scoring in another form of experiment.

The difference between two forms of experiment need not be large in order to show this difference between degree of inhibition.

In a series of $\Psi\gamma$ (ESP) experiments reported in the JOURNAL OF PARAPSYCHOLOGY (3), I found that I got consistently negative scoring in a DT experiment which I had used many times before, chance scoring in a precognition experiment which I had done less frequently before, and consistently positive scoring in a slightly different form of precognition experiment which I had never done before.

This shows the danger of concluding that one experimental method is intrinsically more fruitful than another from the fact that it gives a better score when one starts using it. This may only mean that it is less inhibited because more novel. The practical suggestion emerging from this observation is that it may be a help towards overcoming the effect of decline to have a variety of tests rather than to use one only. There are a good many possibilities: the subject may throw for a series of pre-determined targets working alone, he may do so in competition with someone else, he may do my $\Psi\gamma^{\kappa}$ experiment, or he may do one of the variety of experiments described in Dr. Humphrey's useful handbook (1). Alternation of a variety of the available techniques is likely to give a higher score than the attempt to repeat a large number of times a single form of experiment.

Total Length of Experiment

The fact of the onset of chronological decline sets a limit to the total length of experiment which it is profitable to carry out with any one subject. It is impossible to make any definite rules on this matter since it is clear that different subjects differ very much in the degree to which they are affected by chronological decline. For example, Dr. Soal's first subject, Mr. Shackleton, seems to have carried on a single experiment to a very great length without serious falling off of score (2). If my own results may be taken as a general guide, it looks as if about 30 experimental occasions for any one experiment were enough. Both my IIA and IIB experiments went on for 36 occasions and reference to the graph showing cumulative totals for these experiments suggests that both went on rather too long for maximum significance. Even this length may have only been possible because each session was a short one (3 runs of 24 falls). It is obvious that further research is necessary before one

can make a general rule as to the optimal length of experiment. Provisionally one may consider about 60-90 runs as a length unlikely to produce serious chronological decline. The main point for experimenters to bear in mind is that there is an optimal length beyond which one should not go in testing any one subject with one test and that it is more economical to make a series less than this optimal length than to have it too long.

Length of Each Experimental Occasion

In the effort to make as much use as possible of each occasion on which the subject comes, the experimenter is tempted to give him as many tests as possible on each occasion. My experiments suggest that this is not an economical procedure. In Experiment I, I worked for an hour or more on each occasion and succeeded in inducing a considerable decline after a few occasions on each experiment. It is true that these few occasions included a large number of falls, but I hoped to be able to work more economically by cutting down the amount of experimentation on each occasion. In Experiment II, I had only three runs of each experiment on any one occasion, although for the early part of the series two experiments of this length were performed on each occasion. When IIA was finished and I was doing IIB alone, I was doing only three runs on each occasion.

I then noticed a fact which made me wonder whether even this was not too long. When I had completed the twenty-eighth occasion of IIB I discovered that all my scoring up to date had been on the first run of each occasion. The totals for the first, second, and third runs of each occasion showed deviations from mean chance expectation of +31, −5 and −1 respectively. While there was no positive deviation on the second and third runs, the score on the first run was highly significant. Taking into account the fact that this was not a single observation but the selected best of three, the odds against so large a deviation occurring by chance are about 250 to 1. For this run alone the mean scoring rate is the very satisfactory amount of rather better than +1 per run. It seemed, therefore, that it would be more economical to cut down the length of each occasion to a single run.

This was done on the next 12 occasions with the disappointing result that these 12 occasions gave a total score of 45 hits, three less than mean chance expectation. Reducing the length of occasion from three runs to one did not, therefore, result in the expected improvement in score. I do not understand why this was; it appears as if the superiority of the first run depends on the intention to carry out two more runs afterwards. In a sense, it may be considered that the logic underlying my expectation was faulty. I considered that in replacing three runs by one, I was doing the first run and leaving out the second two; it could be argued that I might as well have considered that I was doing the last one and leaving out the first two. The failure to get a satisfactory score on one run was not due to a general inhibition due to chronological decline since the remaining four occasions of this experiment showed a deviation of +7 on the first run, of +4 on the second run, and of 0 on the third run. Although the numbers are too small for statistical significance, the indication is that positive scoring was resumed and (less strongly) that the old pattern of scoring predominantly on the first run was resumed.

I feel no very strong conviction of the superiority of short experimental sessions on each occasion. Clearly chronological decline does set in even with an experimental session as short as the one I used in Experiment II and I have no clear indication that it does not set in as soon, when measured in terms of total experimenting, when the sessions are short as when they are long. I am still inclined to favor the short session and my present practice is to have not more than six runs of all experiments together for each experimental session. Further research is, however, necessary to establish whether this is really an economical procedure.

Induction of Favorable Psychological Conditions

All psi phenomena depend to a very large extent on the psychological condition of the subject; I am inclined to think that the performance of PK tasks is more easily upset by unfavorable psychological conditions than that of ESP tasks. In self-experimenting, it has seemed to me that I have not scored well either on ESP or PK if I am tired, cross, ill, or anxious. In experimenting with other

subjects I have noticed that positive results tend to disappear if there is any hint of a hostile or suspicious attitude on the part of those present or if tension is increased by over-emphasis on experimental precautions. These, of course, must be adequate but the rule should be: adequate precautions without fuss. If witnesses are present, their attitude should be that of cooperative friendliness.

It is easy to avoid experimenting when the subject is ill, cross, or tired. Anxiety is more troublesome since there is always present a situation to become anxious about—the success of the experiments. Possible ways of dealing with this source of anxiety have been discussed earlier.

As well as trying to avoid these obvious sources of unfavorable conditions for positive scoring, we may consider whether there is any technique that can be used for inducing a psychological condition favorable to positive scoring. It would be ideal if we could find a method which led to consistent positive scoring without chronological decline. I can report only limited success in the search for such a method. My only success in inducing a state of mind which seemed, temporarily at least, to have a real effect on scoring rate, was when during experiment IIA I hit on the device of repeating poetry to myself while the experiment went on. The point at which this device was introduced is shown by the line *A* in the foregoing graph. The immediate effects were striking. The deviations for the first six occasions after this method was adopted were 0, +5, +3, +2, +1, +8 with the total deviation of +19 for 18 runs (i.e., 432 single die-falls). The previous six occasions had given scores: −1, −4, −1, 0, +2, −5, totaling −9. The difference of +28 in favor of the deviations after the change of method is significant (CR=2.56; P=.01).

This looked good and I thought I had discovered a method of overcoming the problem of low scoring. This hope was not fulfilled. That the method is not adequate to overcome the inhibition due to chronological decline is shown by the fact that it did not affect at all my rate of scoring in the thoroughly inhibited hand-thrown series going on at the same time. Moreover, even the series IIA showed distinct evidence of chronological decline in its later stages although the recitation of poetry went on all the time. I continued this

method to the end of Experiment IIB although I was doubtful then whether it was having any useful effect. In experiments carried on later than those reported here I have used it only occasionally with no clear indication that it now makes any difference one way or the other.

It may well be that the initial success of the method was due not to any specific effect of the poetry but rather to the fact that it was a novelty; it may have been another example of an inhibition to a particular form of a test having been overcome by a change in the test. It may, on the other hand, have been due to the fact that the change in psychological attitude induced by the poetry at the beginning was no longer induced later on. I am inclined to think that both factors may have been operative. Certainly the experience of exaltation brought about by the poetry at the beginning was not maintained. It soon came to be part of the rather boring routine of the test, no more exciting than would have been the recitation of the alphabet.

It does, however, suggest a principle of experimenting worth trying out in order to help positive scoring. It may be worth while to try all kinds of emotionally stimulating stimuli such as music, discussion, problem-solving, etc. before or during the test, varying these so that none of them become a merely accustomed routine. I have no doubt that different conditions may be effective for different subjects. The tense atmosphere of the hushed experimental room is likely, on the whole, to prove unfavorable for most of them.

Conclusion

It is obvious that, even at the end of my experiments, I was very far from having solved the problem of how to ensure success. That is clear from the low rate of scoring I had reached at the end. One would like to be able to describe conditions which would ensure that any experimenter could get successful results by adhering to them. That is not possible even for ESP experimentation, and it is my impression that successful PK experimenting is more difficult, since the effect seems more quickly inhibited by continued experimentation and more easily affected adversely by unfavorable psychological conditions. It is not, therefore, a matter for surprise if

other experimenters do not always succeed in getting successful results.

There are, however, clear indications of certain things that they should avoid. One must avoid increasing the emotional tension of the subject by showing gratification at his success and disappointment at his failure. One must not introduce an atmosphere of tension by maintaining silence during sessions. If witnesses are present they must not be allowed to create tension by maintaining a suspicious attitude or by such behavior as whispering. The subject should not be required to do large numbers of experiments at a time or to do a sufficient total number of sessions to make him exasperated by the experiment. A single experiment should not be repeated without variation throughout the experimental series. The satisfactoriness of an experimental design should be judged, at least in part, by the extent to which it avoids all of these unfavorable conditions.

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THE REINFORCEMENT EFFECT IN ESP DISPLACEMENT[1]

By J. G. Pratt

ABSTRACT: In 1936 Dr. S. G. Soal worked with two successful ESP subjects whose extrachance scoring came on the cards immediately preceding and immediately following the one at which a sender was looking when the subject made his call. He found suggestive evidence that this displacement scoring was higher when the preceding and following cards were of the same symbol than when they were different, as though the ESP impression from one displacement reinforced that from the other. In 1942-43 one of these same two subjects, B.S., did a further series of ESP tests which gave evidence of both forward and backward displacement. When these data were analyzed for the reinforcemnt effect, the results appeared to confirm the earlier findings fully. Calls coming between like card symbols scored at about the 50 percent level on displacement, whereas those coming between unlike targets scored less than 25 percent displacement hits (chance expectation being 20 percent in each case).

Prof. M. S. Bartlett, the statistician, pointed out a weakness in the method that had been used in evaluating this effect, and Dr. Soal abandoned the conclusion that reinforcement had been demonstrated. Dr. Pratt, with the aid of Dr. T. N. E. Greville, then took up the question of a satisfactory method of evaluation with Prof. Bartlett. Two statistical methods have been worked out. One was suggested by Mr. A. M. Walker, Prof. Bartlett's associate at Manchester University, and the other by Dr. Herbert Robbins of the University of North Carolina. Both methods give results which indicate that the displacement scores obtained in the reinforcement situations are significantly higher than those in the non-reinforced situations. One of the statistical methods is described in another paper in this same number of the Journal.—Ed.

THIS report will be chiefly concerned with findings that have emerged from the work of Soal and Goldney (4), and a description of their procedure will simplify the explanation of the special effect to be considered. The subject, B.S., selected for his outstanding ESP ability on the basis of earlier tests, was asked to name the order in which five target objects were presented when he had no means of gaining the information through the known sensory channels. The targets were five animal pictures or initials of the animal names, but they may be represented here by the letters A, B, C, D, E. Random lists of the digits 1 to 5 were prepared in advance of each session by the experimenter,[2] each list containing 50 digits, enough

[1] This paper was read at the annual meeting of the Southern Society for Philosophy and Psychology at Roanoke, Va., on March 23, 1951.

[2] These lists were made by taking the last digit of seven-place logarithms at 100-number intervals.

to provide the target order for a set of 50 trials. Before the test began, the subject was seated at a table with an observer, and a sender was seated in another room with a second observer. When all was ready, the experimenter sat down with his first list of 50 random digits before a screen that separated him from the sender. The sender had a set of five cards with the target symbols which he shuffled anew before each set of 50 trials and placed face down in a row before him. The experimenter, who did not know the sender's arrangement of target cards, looked at the first digit on his list and held a card with this number on it at a small window in the screen. The sender saw this card and immediately lifted and glanced at the corresponding target card, counting from left to right. As the sender silently concentrated on the first target, the experimenter said, "One," to signal those in the next room, and the subject wrote down his impression of what target it was; then the sender concentrated upon the next target while the subject wrote his call for it, and so on through 25 trials. After a slight pause, this procedure was repeated for the second 25 trials of the set. The 50 trials were recorded on a single record sheet, and several sheets of data were taken at a session.

A set of 50 trials was recorded on a single record sheet in two columns of 25 trials each. A sample record sheet is shown in Fig. 1. When the call of the subject was the same as the target directly alongside it, this means he made a hit on the card at which the sender was looking. In the sample record sheet, the subject made 6 direct hits in the column on the left and 9 in the column on the right.

We have found, however, that for some subjects the direct hits tell only part of the story of what is going on in the ESP tests. Several indirect or secondary ESP effects have been found, one of which is called displacement. This refers to a tendency of the subject to misdirect some of his calls to the target cards coming just before or just after the ones he is trying to identify. The record sheet in Fig. 1 shows 20 backward displacement hits to the immedi-

RECORD SHEET

(Simplified for purposes of illustration)

Subject: B.S. Date: May 15, 1942

(Space for noting Sender, Observers, Experimenter in charge, Witnesses, Elapsed time per run, Remarks or suggestions, etc.)

Call	Card		Call	Card
C	C		A	A
C	D	Backward displacement hit.	C	B
D	E		A	B
A	B		A	A
B	C		A	D
E	B		D	A
D	D	Direct target hit.	A	E
D	A		E	C
A	D		E	D
E	A		D	A
C	A		C	E
A	E	Reinforcement situation for	C	D
C	A	backward and forward displacement.	B	D
B	C		C	C
C	B	6.....Direct Score.....9	C	C
C	C	9..Backward Displacement..11	C	D
D	D	9...Forward Displacement....8	A	C
E	E		D	E
E	A		E	E
A	D		E	E
B	D		A	B
E	E		A	A
D	E		A	A
B	C		B	A
A	B	Forward displacement hit.	A	A

FIG. 1. Sample record sheet from the Soal and Goldney experiment.

ately preceding card and 17 forward displacement hits to the following card.

The displacement effect may occur in a series of trials with or without significant scoring on the direct targets. An illustration of displacement that occurs in the absence of significant scoring on the direct target is found in this work of Soal and Goldney with the subject, B.S. This series of experiments covered a period from 1941 to 1943. During the first part of these tests B.S. scored at a significantly high rate chiefly upon the target card of the next trial, and only rarely gave a significant score on the direct card, the one at which the sender was looking when he wrote his call. Toward the end of the series, however, a different sender was introduced, and B.S. began to show about equal extrachance scoring on the pre-

ceding target (backward displacement) and the following target (forward displacement) while his direct target score was about the expected chance rate.

The results of these sessions in which the subject showed both forward and backward displacement scoring on the cards one place away are shown in Fig. 2. The call and card situation represented is shown above each bar of the graph. Thus, the middle bar shows the amount by which the direct target score deviated from the expected rate of twenty percent. The bar just to the left of center shows the percentage of successes when each call was checked with the preceding card in the target series. That to the right of center gives the percentage of successes when each call was checked with the next following card in the series. A total of 750 trials were done under these conditions, the equivalent of 30 runs. The critical ratio for backward displacement is 5.88 (P=.000,000,004) and that for forward displacement, 5.50 (P=.000,000,04), so the scoring on the cards one place away in each direction was highly significant.

In some tests the subject was instructed to speed up his rate of calling, which he did by responding at about twice his normal tempo. It was found that at this rapid rate of calling, the displacement success was shifted to the cards two places before and after the ones the subject was trying to name. The displacement, in other words, seemed to remain relatively constant as far as the time interval was concerned, with the result that at the rapid rate the scoring tendencies fixed upon the cards two steps away. In 23 columns done at the rapid rate, the backward displacement score on the second preceding target card gave a CR of 4.89 (P=.000,001) and the forward displacement to the target of the second trial ahead, a CR of 4.67 (P=.000,003). This finding is shown in Fig. 3.

I turn now to a discovery that was made through further study of the work represented by Figs. 2 and 3, the reinforcement effect, with which this paper is particularly concerned. The question arose: Did it make any difference in the displacement scores whether the

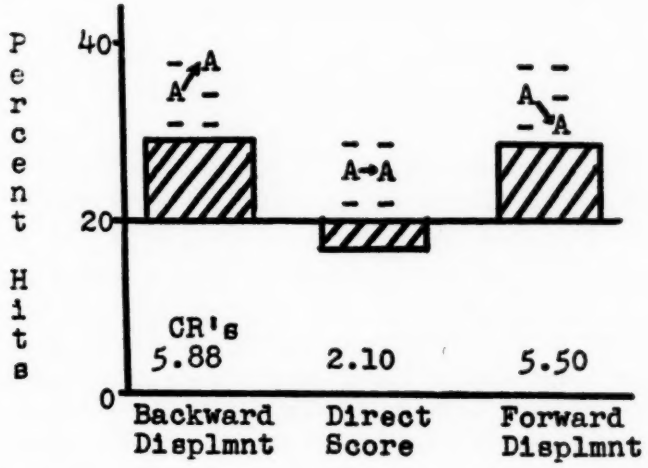

FIG. 2. Thirty ESP runs by subject B.S. (Soal and Goldney, 1943).

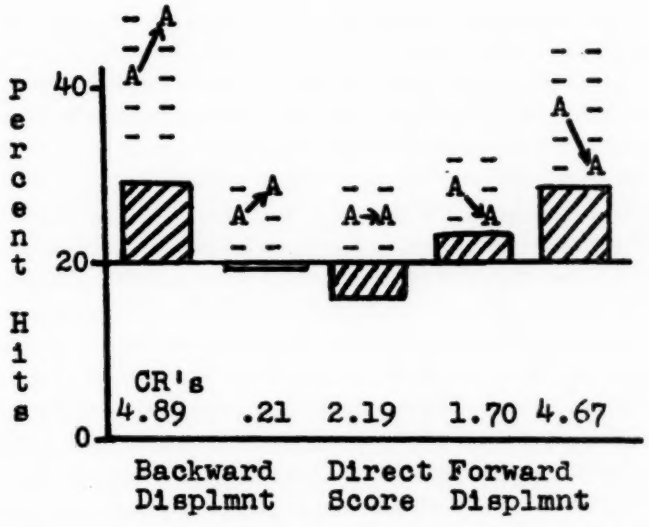

FIG. 3. Twenty-three ESP runs by B.S. at rapid rate (Soal and Goldney, 1943).

forward and backward displacement cards were identical or different symbols? Both situations are shown as follows:

	A			A	
A	—		A or B	—	
	A			B	
Reinforcement condition			Cancelling-out effect		
Call A will result in two displacement hits.			Call A or B will result in only one displacement hit.		

Obviously, when the two cards were the same, backward and forward displacement tendencies could work together, as the subject could score hits on both displacement targets with a single call, and thus "reinforcement" might occur. On the other hand, if the two cards were different the forward and backward displacement tendencies might work against each other and tend to cancel each other out. Preliminary work with B.S. and another subject, Mrs. S., in 1936 in which displacement occurred in both directions had suggested that such reinforcement may occur, and the 1942-43 work of B.S. offered an opportunity to test this hypothesis. Therefore, the instances in which the calls came between two identical target symbols were singled out, and a much higher rate of displacement scoring was found on these than on the remaining cases. Fig. 4 shows the difference between the reinforcement trials and non-reinforcement trials in terms of percentage ratios of hits to total displacement trials for the data of Fig. 2. The multiply-determined calls scored at approximately the 50 percent level, while the non-multiply-determined calls were below 25 percent for both backward and forward displacement. The expected chance rate for both reinforced and non-reinforced calls is 20 percent.

In checking for reinforcement at the rapid rate of calling, the favorable patterns would be expected to be those in which the second cards before and after the direct targets were the same, and the unfavorable ones those in which these two cards were different. Fig. 5, based on the data of Fig. 3, shows that this prediction was borne out in a very striking manner. Again, the rate of success on the multiply-determined patterns was about 50 percent while the rest

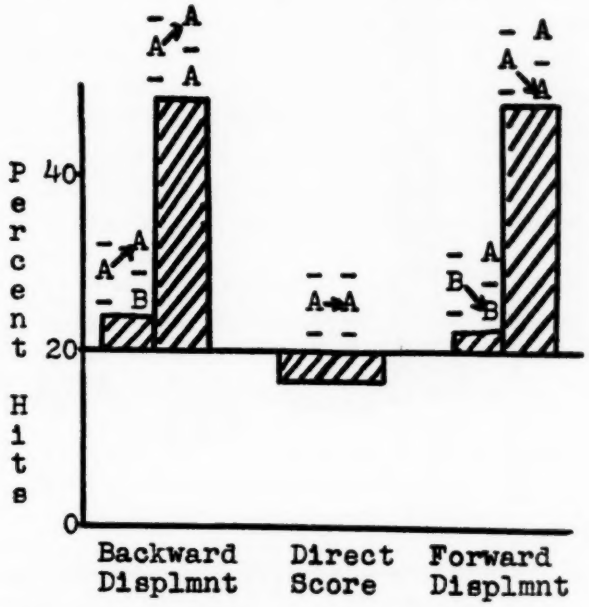

FIG. 4. Reinforcement of displacement (data of Fig. 2).

of the calls scored around 25 percent hits. It is clear that the reinforced trials gave a much higher rate of displacement scoring.

This effect has had an interesting history in the literature. After Dr. Soal had announced the reinforcement finding on the basis of his 1936 work, and he and Mrs. Goldney had reported its confirmation in the 1942-43 series, Professor M. S. Bartlett, the statistician, offered a criticism of the chi-square test of significance that had been used (1). This criticism arose from the fact that the target patterns which overlapped (as when a sequence of three even-numbered targets is scored as two reinforcement patterns) were not statistically independent of one another. Dr. Soal accepted the criticism and made another evaluation which he recognized as approximate only and which gave only marginally significant differences favoring the reinforcement results (3). Dr. Soal thereupon abandoned the conclusion that reinforcement was an established effect in simultaneous forward and backward ESP displacement.

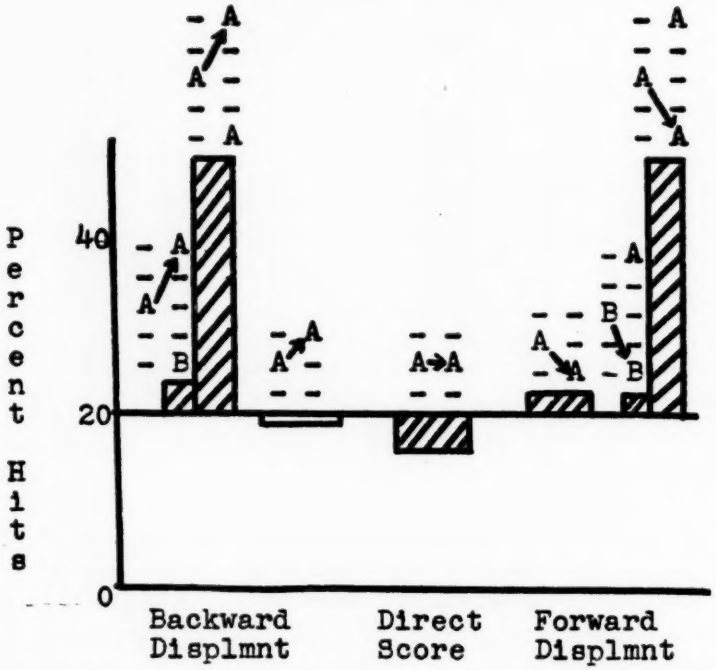

FIG. 5. Reinforcement of displacement at rapid rate of calling (data of Fig. 3).

It is at this point of the discarding of the reinforcement hypothesis that my own part in the study of the effect begins. Doubtful of the adequacy of the grounds for the rejection of the finding, I undertook a reexamination of the evidence and found that Dr. Soal had made an error in applying his statistical formula for getting an approximate evaluation of the reinforcement data and had considerably underestimated the significance of the results. Dr. T. N. E. Greville and I then took up the question of finding an adequate method of evaluation with Professor Bartlett and his associate, Mr. A. M. Walker, and we finally arrived at a satisfactory statistical test which shows that the reinforcement effect is highly significant.[3] This method, which is described by Dr. Greville in the following

[3] I am greatly indebted to Dr. Soal for making copies of his experimental data available for this investigation.

article in this number of the JOURNAL, has been applied to the four series of data available in which both forward and backward displacement showed a positive rate of scoring. Following the terminology of Soal (**2**) and Soal and Goldney (**4**), we may refer to the instance in which a call falls between identical targets as a "multiply-determined pattern" and one with the call between non-identical targets as a "non-multiply-determined pattern." In the rapid rate tests, of course, it is the targets two steps removed that define the patterns.

In the evaluation of results for a reinforcement effect, statistical independence of observations is assured by scoring each target in only one pattern. Each target is thus scored for either backward or forward displacement depending upon whether it comes at the start or at the end of the pattern.

The method of evaluation described in the following article gave the following results on the four sets of data:

(1) In the B.S. normal rate tests of 1942-43, there were 552 non-overlapping pairs of targets to consider. In 121 of them the two targets were the same and 60 of the calls were correct (i.e., each call scored a double hit). On the remaining 431 non-multiply-determined patterns, 202 calls were correct (i.e., each one scored a single hit, either backward or forward displacement). These figures give a chi square of 14.18 (2 degrees of freedom), with P=.0009.

(2) For the application of the method to the B.S. rapid rate data of 1942-43, non-overlapping patterns were obtained by checking each of the following calls within the column against the second preceding and following target cards: 3, 4, 5, 6; 11, 12, 13, 14; and 19, 20, 21, 22. The decision to use these particular patterns was made before the tally was started. The tally gave a total of 276 non-overlapping patterns to be considered. There were 60 calls on multiply-determined patterns, with 35 of them giving double hits. On the other hand, there were 216 calls on non-multiply-determined patterns, with only 95 of them scoring single hits. The evaluation of these data for reinforcement yields a chi square of 18.42 (2 d.f.), with P=.0001.

(3) In the B.S. data from the 1936 series, there were 384 non-overlapping patterns. Of these, 76 were multiply-determined patterns, with 29 of the calls giving double hits. On the 308 non-multiply-determined patterns, 130 single hits were scored. These data give a chi square of 5.64 (2 d.f.), with P=.07, which is only suggestive of a reinforcement effect.

(4) The other series, the 1936 work with Mrs. S., amounted to 960 non-overlapping patterns. There were 168 multiply-determined cases, with 46 calls giving double hits, and 792 non-multiply-determined calls, 355 of them being correct for one of the two symbols. The chi square derived from these figures is .06, which is not significant.

This paper and the following one by Dr. Greville were read in manuscript form by Dr. Herbert Robbins of the Department of Mathematical Statistics, University of North Carolina. Dr. Robbins pointed out that the method of evaluation proposed by Mr. Walker was based upon the assumption that ESP operates to give true cognitions on some trials and that chance is effective on the remainder of the trials. While this assumption may seem to some people to be the most reasonable one to make, Dr. Robbins took the view that it is better not to introduce any unnecessary assumptions. He argued that there are a large number of different assumptions which might be made on a priori grounds. For example, it is possible to assume that on each trial the subject knows by ESP one symbol which is not the target and he calls one of the other four symbols with a ¼ probability of success. The use of any of the possible assumptions would lead to a different formulation of the statistical problem and therefore to a different result. Such an approach is therefore a test of the underlying assumptions, as well as a test of the reinforcement effect. If a significant result is obtained under these circumstances, the reason may be that there is a reinforcement effect in the data, but it could just as well be that the underlying assumptions were incorrect.

Dr. Robbins suggested that the following method offers a more direct approach to the evaluation of the reinforcement effect inasmuch as it involves a direct test of the probabilities of hits in the

multiply-determined and non-multiply-determined patterns. Let us say that we are interested in testing whether there is a difference in the probability of occurrence of a forward displacement hit in the two types of patterns. We will consider only those non-overlapping pairs that were tallied for the previous evaluation so as to avoid all question of the lack of independence among separate observations. By arbitrarily confining ourselves to the forward displacement hits, we avoid the difficulties which arise from combining both directions of displacement in a chi-square test.

The results for the four series of data by this method of evaluation are shown in Table 1. The level of significance of each series agrees quite closely with that obtained from following the method described by Dr. Greville. The outcome of the two different evaluations may, therefore, be summarized in the same general terms. Taking the series in their approximate chronological order, we find that Mrs. S. showed no evidence of reinforcement in 1936, that B.S. gave suggestive evidence of the effect at that time, and that he produced quite significant evidence of reinforcement both at the normal rate and at the rapid rate in 1942-43. The significance of all the reinforcement results may be obtained by combining the probabilities from the four separate series by Fisher's method. This gives a probability value of $<.0002$ for the results as a whole by the Walker-Greville method and $<.000003$ by the method suggested by Dr. Robbins. The fact that the forward displacement hits were selected for use in the second evaluation does not imply that the higher probability of making a hit in a multiply-determined pattern applies only to that direction of displacement. The fact that the difference between the two pattern situations studied is equally large for forward and backward displacement (see Figs. 4 and 5) clearly shows that the reinforcement effect is shared by both types of displacement.

While the evidence for reinforcement is incidentally first rate confirmation of the occurrence of ESP, we are past the point now of primary concern over that question, and I will give attention rather to the question of the psychological significance of this displacement effect—its bearing on the nature of ESP. Displacement, considered as a misdirected response, appears to be one of the effects of the

Table 1

THE EVALUATION OF THE FORWARD DISPLACEMENT HITS IN NON-OVERLAPPING PATTERNS IN THE FOUR EXPERIMENTAL SERIES
(Expected values in each cell are shown in parentheses.)

Mrs. S., 1936

	Patterns M.D.	Patterns N.M.D.	
Hits	46 (49.1)	185 (181.9)	231
Misses	122 (118.9)	437 (440.1)	559
	168	622	790

$$\chi^2 = \frac{[(46)(437) - (185)(122)]^2 [790]}{(231)(559)(622)(168)}$$

$= .36$ (1 d.f.)

$P = .55$

B. S., 1936

	Patterns M.D.	Patterns N.M.D.	
Hits	29 (21.9)	62 (69.1)	91
Misses	47 (54.1)	178 (170.9)	225
	76	240	316

$\chi^2 = 4.28$ (1 d.f.)
$P = .04$

B. S., 1942-43 (Normal Rate)

	Patterns M.D.	Patterns N.M.D.	
Hits	60 (43.4)	102 (118.6)	162
Misses	61 (77.6)	229 (212.4)	290
	121	331	452

$\chi^2 = 13.58$ (1 d.f.)
$P = .0002$

B. S., 1942-43 (Rapid Rate)

	Patterns M.D.	Patterns N.M.D.	
Hits	35 (21.9)	49 (62.1)	84
Misses	25 (38.1)	121 (107.9)	146
	60	170	230

$\chi^2 = 16.66$ (1 d.f.)
$P = .00005$

unconsciousness of the ESP process. The subject seems to have no introspective awareness of the operation of ESP in relation to success and failure in the tests by which he can direct his responses and on the basis of which he can profit from experience. Successful subjects have made repeated efforts to single out their correct calls. In only a few subjects has the rate of scoring on such marked calls been better than that on the unmarked ones, and further investigation is needed to settle the question of whether ESP can be identified

introspectively. Under the circumstances, it is not surprising that displacement occurs.

ESP research has advanced to the point that the most pressing problem is now that of gaining greater control over the process. This end might be accomplished by finding some way by which the subject can bring his ESP function within the scope of awareness. If this could be done, we could rely upon the calls of the subject to be correct when he said they were, even though we might have to wait a long time between genuine impressions.

The possibility remains, however, that the process is necessarily unconscious in its operation and that some other avenue of approach must be followed in achieving a larger measure of control. For this reason, any finding that suggests that a subject is able to achieve a higher level of ESP scoring under some special situation or condition is an important clue to be followed up in further research.

The reinforcement effect in displacement is one of the most striking instances of improvement in ESP performance that has been observed. If we can find out what caused B.S. to do so much better when he had the particular situation in the target order presented by a multiply-determined pattern, we may be able to generalize to other situations and bring about similar improvement in scoring in other subjects, even those for whom displacement is not the primary ESP effect.

One thing reinforcement indicates is that ESP is not an all-or-none act of perception comparable to, say, visual perception under conditions that are either quite favorable or totally unfavorable to sight. If the perception were a complete perception of the correct target in every case in which it occurs, there would be no need for reinforcement and no opportunity for reinforcement to show up in the results. We cannot now say, of course, at what point in the response the limitation is imposed—whether it is a difficulty of establishing contact by ESP with the mental or physical event set up as the target or one of bringing the ESP impression to expression in overt, observable behavior, or both. Indeed, we cannot be sure, in the present state of our knowledge, that anything we may now say about the problem will make sense later when we have a great many more facts to consider.

The reinforcement finding also emphasizes another fact that may be of outstanding importance for ESP research. These results show that there are configurational features of the target situation that affect ESP performance and which justify the hope that further progress can be made toward the understanding of ESP by studying it as an act of *perception.* This is not to say, of course, that previous findings offered no such hope, but merely that this discovery opens up a new avenue which may lead to advances in knowledge regarding the psychology of psi processes and may help to relate them to more familiar aspects of behavior. It is important to remember, however, that the present study does not *measure* the reinforcement effect nor tell us what type of influence from the targets accounts for the observed difference in rates of scoring. The results only justify the conclusion that the difference did not arise through chance variation. There is a possibility that there is an improvement of scoring rate in the reinforced patterns and a lowering of the rate, due to something like a cancelling-out effect from the two unlike targets, on the other trials. In fact, in earlier work by B.S. in which there was extrachance scoring on forward displacement only, the average was around 30 percent. That average lies between the rates for the two types of patterns analyzed here, so that the difference noted in this study seems to result from both a raising of the scores in the multiply-determined patterns and a lowering in the non-multiply-determined patterns. Until more light can be thrown upon the finding through further research, the term reinforcement should be regarded as one of convenience and not as an explanation of the effect.

Further work along these lines is already under way. Dr. Soal has done a very extensive series of tests with Mrs. S., his second high-scoring subject, during the past five years. At the present time (May, 1951) he is visiting the Parapsychology Laboratory as a Fulbright Exchange Scholar to carry out research upon these unpublished records. Our chief aim is to throw further light upon some of the displacement effects in these data, effects that appear to be related to the reinforcement finding.

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MINOR ARTICLES

A METHOD OF EVALUATING THE REINFORCEMENT EFFECT

By T. N. E. GREVILLE

IN INVESTIGATING the occurrence of a reinforcement effect, one is interested in call-card patterns consisting of either an even-numbered call and the preceding and following odd-numbered cards, or an odd-numbered call and the preceding and following even-numbered cards. In discussing the evaluation of a possible reinforcement effect, Professor M. S. Bartlett has pointed out that if patterns containing overlapping pairs of cards (for example, the pair consisting of the first and third cards of a run and the pair consisting of the third and fifth cards) are used, there is a lack of independence which renders the usual methods of statistical evaluation not strictly applicable (1).

For the purpose of the evaluation, therefore, we decided to consider a set of non-overlapping alternate pairs of target cards selected in the following manner. In each run, the first and third cards are taken as the first odd pair, the fifth and seventh cards as the second odd pair, and so on. The second and fourth cards are taken as the first even pair, the sixth and eighth as the second even pair, and so on. The overlapping pairs not included in this selection, such as, for example, the third and fifth, or the fourth and sixth cards of a run, are not considered or used in any way. This means that, in a sense, 50 per cent of the information is wasted. However, the non-overlapping target pairs used may be regarded as independent for evaluation purposes.

Among the non-overlapping pairs thus selected, a tally is made of the number of multiply determined and non-multiply determined patterns and the number of hits obtained in each of these categories. (In this discussion a hit on a multiply determined pattern refers to a call that matches both target symbols, while a hit on a non-multiply determined pattern indicates that the call matches one of

the two targets involved.) The problem now arises of determining what number of hits would be expected in each case if there is no "reinforcement" effect. The method to be described for obtaining these numbers has been suggested, in private correspondence with the author, by Mr. A. M. Walker of Manchester University, a colleague of Professor Bartlett. On those patterns where ESP does not occur, the probability of a hit (precognitive or postcognitive or both) is ⅕ for a multiply determined pattern, and ⅖ for a non-multiply determined pattern. Let p denote the probability that a hit (precognitive or postcognitive or both) occurs by ESP on a given call. On the null hypothesis that there is no "reinforcement" effect, this probability would be the same for multiply determined and non-multiply determined patterns. Then the *total* probability of a hit (whether by chance or ESP) is $p + \frac{1}{5}(1-p) = \frac{1}{5} + \frac{4}{5}p$ for a multiply determined pattern, and $p + \frac{2}{5}(1-p) = \frac{2}{5} + \frac{3}{5}p$ for a non-multiply determined pattern. If r denotes the probability of a multiply determined pattern, the probabilities of hits and misses on multiply determined and non-multiply determined patterns are as indicated in Table 1.

Table 1

PROBABILITIES OF HITS AND MISSES ON MULTIPLY DETERMINED AND NON-MULTIPLY DETERMINED PATTERNS

	Multiply determined	Non-multiply determined
Hits	$r(\frac{1}{5} + \frac{4}{5}p)$	$(1-r)(\frac{2}{5} + \frac{3}{5}p)$
Misses	$\frac{4}{5}r(1-p)$	$\frac{3}{5}(1-r)(1-p)$

Table 2

NOTATION FOR OBSERVED FREQUENCIES OF HITS AND MISSES ON MULTIPLY DETERMINED AND NON-MULTIPLY DETERMINED PATTERNS

	Multiply determined	Non-multiply determined
Hits	n_1	n_2
Misses	n_3	n_4

If the observed frequencies in the four cells are denoted by the symbols indicated in Table 2, the likelihood L of Table 2 is given by:

$$\ln L = n_1 \ln \left[r \left(\frac{1}{5} + \frac{4}{5} p \right) \right] + n_2 \ln \left[(1-r) \left(\frac{2}{5} + \frac{3}{5} p \right) \right] + n_3 \ln \left[\frac{4}{5} r (1-p) \right] + n_4 \ln \left[\frac{3}{5} (1-r)(1-p) \right]$$

The maximum likelihood estimates of r and p are obtained by taking partial derivatives with respect to these two variables and equating them to zero. This gives the two equations:

$$\frac{n_1}{r} - \frac{n_2}{1-r} + \frac{n_3}{r} - \frac{n_4}{1-r} = 0,$$

$$\frac{4n_1}{1+4p} + \frac{3n_2}{2+3p} - \frac{n_3+n_4}{1-p} = 0.$$

Simplifying these equations gives:

$$r = \frac{n_1+n_3}{n_1+n_2+n_3+n_4},$$

$$12(n_1+n_2+n_3+n_4)p^2 - \left[4n_1+9n_2-11(n_3+n_4) \right] p - \left[8n_1+3n_2-2(n_3+n_4) \right] = 0.$$

If $n_1 + \frac{1}{2} n_2 \geqq \frac{1}{5}(n_1+n_2+n_3+n_4)$, which will always be the case if scoring has been above the chance level, the second equation always has a single positive root between 0 and 1. This is, of course, the desired maximum likelihood value of p, which may be denoted by $\hat{p}$.

Substituting the expression obtained for r, the expected frequencies in the four cells of the table are found to be as given in Table 3.

Table 3

EXPECTED FREQUENCIES OF HITS AND MISSES ON MULTIPLY DETERMINED AND NON-MULTIPLY DETERMINED PATTERNS

	Multiply determined	Non-multiply determined
Hits	$\left(\frac{1}{5}+\frac{4}{5}\hat{p}\right)(n_1+n_3)$	$\left(\frac{2}{5}+\frac{3}{5}\hat{p}\right)(n_2+n_4)$
Misses	$\frac{4}{5}(1-\hat{p})(n_1+n_3)$	$\frac{3}{5}(1-\hat{p})(n_2+n_4)$

This method of evaluation of course assumes that the probability of a hit by ESP is constant from trial to trial and further that the probability of success on any trial is independent of the results of other trials. It is obvious that these assumptions may be questioned.

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PSI EXPERIMENTS WITH AUSTRALIAN ABORIGINES

By Lyndon Rose and Ronald Rose

ABSTRACT: The subjects in these experiments were semi-civilized, half- and lower-caste natives of New South Wales whose cultural background included a wide acquaintance with, and general acceptance of, spontaneous psi phenomena. Twenty-three subjects were tested for GESP, with the sender and receiver in separate rooms, or with one inside and one outside a building. Two experimenters conducted the ESP tests: one shuffled the cards and observed the sender or acted as sender himself, and the other recorded the receiver's calls. A total of 296 runs, carried out with standard ESP cards, gave highly significant results, the greater part of which were contributed by one subject. The scores of this subject were unusual in two respects: there was a significantly "greater-than-chance" number of hits on the direct target and a significantly "below-chance" number of displacement hits to the cards one place before and one place after the ones at which the subject was aiming.

There were no significant results from PK tests with colored cubes in which 20 subjects threw 1,896 runs for target faces of a specified color.

The authors point out the need for further psi tests with the aborigines and stress the importance of carrying them out before the native culture deteriorates.—Ed.

Introduction

In August, 1949, the writers visited Woodenbong Aboriginal Settlement on the North Coast of New South Wales and carried out some conventional GESP and PK tests with the aborigines there.[1] The natives at Woodenbong, mostly half- and lesser castes, are not primitive or tribal, although some of them have experienced such conditions. As a group they have reached the stage of absorption into the white community, and they present, consequently, a difficult social problem. They are confused by the clashing principles of the culture they are abandoning and that which they are tending to embrace. In particular they are confused by the differences between their own magico-religious outlook and that propounded by a variety of missionaries representing different denominations.

They tend to regard psi matters as belonging almost solely to the culture of the past. The idea of telepathy and the experience

[1] The experiments were made possible through the approval and cooperation of the Aborigines' Welfare Board and its officers, and through the advice of Professor A. P. Elkin of the Department of Anthropology at the University of Sydney, to all of whom the authors extend their thanks.

of it were completely familiar to the older natives interviewed, although the younger ones, whose association with white people has sophisticated them, scoffed at the idea. The latter conceded that probably telepathic events occurred in the past but regarded the idea generally as a superstition. It was the older natives who claimed to have constantly experienced episodes of a telepathic nature and to have observed the powers of clever men (native doctors, magical practitioners) in tribal or semitribal existences some years ago. Some of them still have association with clever men and apparently supernormal phenomena at the present time (**4**). Their belief is that clever men have telepathic and other paranormal powers at all times at a conscious level, but that others have psi experiences only occasionally. Those occasions on which other than clever men are acknowledged to show telepathic ability are frequently "crisis cases" in which the death or serious illness of a distant relative is involved. All the older and a good many of the younger natives asserted with positive assurance that they would, without doubt, become aware of the death or serious illness of a distant relative. This was regarded as a reliable and more or less commonplace experience. The absoluteness of their belief in this process was impressive.

Some difficulty was had in describing the use of cards to detect extrasensory perception and it is thought that some natives, in the early stages at least, went through the mechanics of guessing without any understanding of the fundamental idea behind the tests. However, most of the natives were quick to grasp the idea and were fully cognizant of the nature of the test. Indeed, it was possible in a few instances to outline in simple terms the statistical methods employed. Considerable emphasis was placed by the writers on the instances of spontaneous telepathy with which the natives were acquainted and they were assured that the cards would show "how good" their telepathic power was.

After the nature and purpose of the tests had been explained, the natives stated, with varying degrees of confidence, that they might be able to demonstrate telepathy by means of the cards. The novelty of the situation appealed to them and they showed a good deal of enthusiasm. They also expressed great faith in the ability of clever men to name unseen cards correctly on each occasion! No clever

man was available at the time, but arrangements are in hand to carry out tests with clever men in the future.

Some of the older natives were highly motivated by a desire to demonstrate telepathic ability by means of the card tests since the writers' inquiries were apparently the first earnest ones in connection with phenomena regarded seriously by the natives but generally ignored or scoffed at by white people, particularly missionaries. Younger natives were attracted by small rewards for good scores (cigarettes and tobacco) and by the novelty of the situation. We continually exhorted the subjects to a high pitch of enthusiasm, lavishing praise on them for scoring in excess of mean chance expectation and glibly excusing them when they scored below. It is thought that in one or two instances this rather exhausting procedure had effect—in PK as well as in ESP. In any case noticeable declines in scoring occurred at times when the experimenters' enthusiasm waned.

ESP Tests

Conditions and Precautions

All the trials were of the general extrasensory perception variety in which someone was looking at the card the subject was trying to call. The cards used were standard ESP decks obtained from Duke University. The agent and the percipient were separated throughout the series by being (a) in different rooms in the same building or (b) one inside a building and the other outside. R.R. either was the agent himself or had the agent under strict supervision during the tests, while the percipient was under the supervision of L.R., who recorded the calls.

All tests were undertaken voluntarily by the natives. Because of the short time at our disposal and the uncertainty as to how many subjects would turn up, and for what period, it was not possible to predetermine the length of the series. The tests were arbitrarily terminated at the end of the time available.

In each experimental situation, whether the location was the house of a subject or the building at which most of the tests were conducted, the possibility of sensory clues of any sort being obtained by the percipient was thoroughly investigated. Subjects and in-

evitable onlookers were instructed that strict silence must be observed as each run was being made, and with one or two minor exceptions, this condition was complied with. However, one run was arbitrarily cancelled when an agent called to the percipient in dialect. Another (yielding a score of 4) was cancelled when a serious interruption disturbed all present.

Care was taken to ensure that no onlookers were in a position to observe the target card and convey information by signs or otherwise to the percipient. No attempt at deception of any sort was observed. The natives showed ready recognition of the importance of the experimental conditions when these were explained, and they adapted themselves with good humor to what were at times rather tedious arrangements.

As a general rule, the agent heard the responses of the percipient, and these served as a guide for the turning of the cards. This being so, R.R., was particularly strict, both when he was observer and when he was himself the agent, to make sure that there was no rearrangement of the cards by the agent so that they would conform with the percipient's calls.

Elkin has pointed out (1) the extraordinary hyperacuity of hearing of Australian aborigines. Consideration was given to this possibility in conjunction with the possibility of unconscious whispering. Agents, percipients, and observers were closely watched for any evidence of this sort. In the case of the principal subject, Lizzie Williams, steps were taken to test her with none but the writers present, and no diminution in her scoring rate resulted. It is recognized, however, that this is an introductory study with aborigines done under field conditions, and the writers hope that the findings can be confirmed and extended by further work with these and other primitive people.

The cards were thoroughly shuffled by R.R. between runs. They were always shuffled and cut out of the percipient's sight. At the beginning of each run R.R. called "Right" and L.R. instructed the percipient to begin calling. During each run R.R. made a mental note of the number of hits. At the end of each run he took the deck to L.R., and the order of the cards was recorded against the percipient's calls. If there was any discrepancy

between the total hits mentally noted and those recorded the deck was again checked through before being reshuffled.

The writers are satisfied that throughout the series and without exception percipients were adequately isolated from sensory knowledge of the cards and that, by virtue of the experimental conditions, it is proper to examine the data presented for evidence of ESP.

Results

Twenty-three subjects were tested for ESP and their results are given in Table 1. There was a total of 296 runs, with a deviation of +226, which has a CR of 6.57. This highly significant total was almost entirely due to the work of one high-scoring subject whose results will be discussed in detail. Eighteen of the 22 subjects, who completed less than 20 runs each, are lumped together under "Remainder." None of the 18 subjects showed any individual scoring trends that approached statistical significance. One other subject, Bert Mercy, also did less than 20 runs. His score of 95 hits in 14 runs, with a CR of 3.34, is listed separately in the table.

Table 1

GENERAL RESULTS

Subject	Age	Runs	Hits	Dev.	SD	CR
Lizzie Williams	75	68	489	+149	16.49	9.03
Clara Williams	68	36	194	+ 14	12.00	1.17
Nellie Charles	20	35	173	− 2	11.83	0.17
Frank Mitchell	39	22	124	+ 14	9.38	1.49
Bert Mercy	62	14	95	+ 25	7.48	3.34
Remainder	..	121	631	+ 26	22.00	1.18
Total, All subjects		296	1707	+226	34.42	6.57

The Principal Subject (Lizzie Williams)

The single subject who produced an outstandingly significant result is an old lady named Lizzie Williams, a half-caste who is a diabetic and a cripple. Some of her best scores occurred while she alternated runs with holding and talking to a somewhat unruly two-year-old great-grandson.

Lizzie Williams carried out 68 runs, with a deviation of +149 and a CR of 9.03. Table 2 shows her chronological decline in scor-

ing. The probability that a difference as great as that between the first and second quarters would have occurred through random variation is less than .005, and the difference between the first and fourth quarters is even greater. When the subject's runs were pooled and then separated into five-trial segments within the run, the deviations for the segments were: +41, +26, +36, +24, +22. These data resemble performance curves previously discussed by Pratt (2) in connection with PK results; that is, the curve shows beginning salience but no ending salience. The differences between the segments are not quite significant statistically, though they have a strong suggestive value since they are consistent with scoring trends often reported in the literature.

Table 2

CHRONOLOGICAL DISTRIBUTION OF HITS IN DATA OF LIZZIE WILLIAMS

	Runs	Hits	Dev.	SD	CR
First quarter.....	17	145	+60	8.25	7.27
Second quarter.....	17	112	+27	8.25	3.26
Third quarter.....	17	124	+39	8.25	4.73
Fourth quarter.....	17	108	+23	8.25	2.79

Probably the most remarkable feature of this subject's performance aside from the positive deviation on the direct target is in displacement effects.[2] Forward and backward displacement to two positions is shown in Table 3.

The significant deficiencies in scoring when the call is compared with the cards on either side of the target position indicate a strong psychological factor at work, one that may be related to that discussed by Soal (5) in connection with his experiments in card guessing. It is difficult to imagine how a subject could score so highly on the target face while at the same time suppressing or in-

[2] The displacement results shown in Table 3 do not take into account the influence of the use of the closed deck, of the positive score on the direct target, and of the subject's individual tendency to call doubles. The displacement forward and backward one position was therefore rechecked for those areas of two or more successive trials that did not show any direct hits, and the displacement results were evaluated by a method that makes allowance for the above factors (3). The displacement results were found to be significant by this method, the CR for the forward displacement being −2.83 (P= .005) and that for backward displacement, −3.64 (P=.0003).—Ed.

Table 3

DISPLACEMENT RESULTS IN DATA OF LIZZIE WILLIAMS

	Runs	Hits	Dev.	SD	CR
Backward, two positions....	62.6	284	− 29	15.80	1.84
Backward, one position.....	65.3	276	− 50.4	16.16	3.12
Target face.....	68	489	+149	16.49	9.03
Forward, one position.....	65.3	279	− 47.4	16.16	2.93
Forward, two positions.....	62.6	290	− 23	15.80	1.46

hibiting displacement successes. It should be pointed out that the writers emphasized before and during the series that only hits on the card the agent was at the time looking at were valid and that the ones before or the ones after were "no good." However, it is not suggested that this fact alone could be responsible, and the existence of a deeper, perhaps fundamental, psychological factor seems more probable.

No other subjects showed marked displacement deficiencies or excesses or other features worth any special attention. Of the principal subjects, only one (Clara Williams) produced a performance curve at all resembling the characteristic U-curve, with five-trial segment scores of 42, 32, 33, 38 and 49 for all runs pooled.

PK TESTS

Conditions and Precautions

Conventional dice were not used in the PK tests. Plastic cubes, 5/16 inch in size were accurately shaped by an engineer (Mr. A. E. Fordham) and their antipodal sides colored black and white, red and yellow, and blue and green. Twelve such cubes were used throughout. Towards the end of the series the colors at the edges of the cubes wore off to some extent but there is nothing to suggest that any bias resulted.

The subjects threw the 12 dice by hand from a shaker lined with corrugated rubber. Tests showed that it was not possible to slide dice from the shaker or to influence their fall by skillful throwing. The dice were thoroughly shaken by the subjects and thrown onto a blanket-covered table where they rolled freely. Simple rules such as rethrowing all dice if any were cocked or fell from the table were

applied consistently throughout. The experimenter replaced the dice in the shaker so that none could be "palmed" by the subject.

All throws were witnessed by both experimenters, one of whom (usually R.R.) supervised the throwing and called results to the other, who checked and recorded. Only hits were recorded.

Clear-cut instances of "mind over matter" are comparatively rare in aboriginal psychic lore, and are regarded as solely the prerogative of clever men. The latter are believed to be able to do such things as call up and disperse storms, perform psychosomatic magic—that is, killing and curing by psychic means (more probably suggestion)—use a "magic cord" for climbing trees, and so on. No special confidence in their own ability to influence the fall of dice was indicated by the subjects, but on the other hand, considerably more interest was shown in PK than in the card tests.

As with ESP it was not possible to predetermine the length of the series.

Each record sheet provided for 24 runs, four for each color, and each subject threw for each color an equal number of times for each page in the order chosen by him.

The use of bright colors rather than conventional dots on the dice attracted the natives, who were particularly intrigued with the idea of throwing for colors, an idea that might well be tried in series other than with native peoples.

Results

Twenty subjects, each of whom did a minimum of 24 runs with the dice, gave a total of 1,896 runs, on which there was a deviation of +128, and a CR of 1.61. As in the ESP tests, one subject completed far more PK runs than any of the others and was the highest scorer. The subject was Nellie Charles, granddaughter of Lizzie Williams, the highest scorer among the ESP subjects. Nellie Charles scored a deviation of +108 in 600 runs (CR = 2.41) which might be considered of marginal significance if it stood alone. Diluted by the large numbers of subjects tested, this score is of suggestive value only. This subject's results present no other features of interest. Her performance curve, based on the six sets of the page, is erratic and unlike those reported from other experiments,

and the chronological distribution of hits revealed no regular trend in scoring.

Conclusion

There is evidence in these tests, though it is not by any means conclusive, that there is some foundation for the anecdotal accounts of extrasensory ability associated with Australian aborigines. If this ability exists it is probably more striking in its spontaneous manifestations than it is in the formal ESP tests, and most pronounced in those natives who have been the least affected psychologically by culture contact. Although several subjects produced scores suggestive of ESP ability, only one native showed particularly marked ability over a substantial number of runs.

Because of the natives' belief that the power of mind over matter is largely the prerogative of witchdoctors, we did not feel that the psychological conditions were favorable for a demonstration of PK by means of dice. The evidence obtained is only *suggestive* of PK ability.

The approach of the experimenters to the psychic lore and abilities of these people was a sympathetic one. A good deal of time was spent in becoming familiar with their concepts of psychic powers and the beliefs associated with them. The dialectal names for psychic occurrences were acquired so that test requirements could be explained in terms familiar to them. These primary requirements having been met, it must be said that the natives were found to be cooperative, enthusiastic, and honest to the point of supererogation.

It is suggested that where the opportunity exists further tests should be carried out by other experimenters, not only with Australian aborigines, but with other primitive people. So far as Australian aborigines are concerned, the writers regard it as imperative that further tests be conducted while the opportunity still exists, and that independent confirmation of the above results would be desirable. It is thought highly improbable that significantly positive results can be obtained experimentally without enthusiasm and at least the appearance of genuine belief in the natives' powers on the part of the experimenter. Merely to go through the mechanics of psi tests would be useless.

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PLACEMENT PK TESTS WITH THREE TYPES OF OBJECTS

By Louisa E. Rhine

ABSTRACT: The main objective of this experiment was to have the subject try by PK to make falling objects come to rest on a designated area of a target surface. For this purpose the experimenter introduced a target surface divided into two equal areas to see if the subject could succeed more easily with this simple target arrangement than with the complex target system used by Cox in his previous placement experiment. The objects used were marbles, coins, and cubes, and they were thrown mechanically from a specially designed apparatus.

A total of 113,100 objects were thrown, with total results that are not statistically significant. The subjects were divided into a miscellaneous group and a special-subjects group. Analyses show several suggestive though not statistically significant internal effects in the data of the latter, which the experimenter tentatively attributes to a more favorable psychological atmosphere. These effects are the basis of suggestions for future research.—Ed.

An innovation in PK research was reported in the March issue of this Journal by W. E. Cox[1] who carried out "placement" PK tests. In contrast to the previous PK tests in which subjects tried to control the die face that fell uppermost, Cox's subjects attempted to "will" falling objects to land on certain designated target areas. Cox's arrangement of target areas was fairly complex in that it consisted of 252 squares, numbered from 1 to 6, and on each trial the subject's "targets" were the 42 squares bearing a designated number. The results obtained with that degree of complexity raised the question of whether or not a subject could succeed more easily with a simpler target arrangement. As a result, the following experiment using only two target areas was undertaken. The tests were carried out between July, 1948, and April, 1949. They did not yield significant results as a whole, but certain suggestive features were noted which may be of sufficient value to future workers to justify a brief report.

The main objective of the experiment was to test the effect of a simple two-target system in which objects released from a fixed point above a centrally divided area were "willed" to fall to the right

[1] "The Effect of PK on the Placement of Falling Objects," *J. Parapsychol.*, March, 1951.

or left, as specified beforehand. The experimental plan required that both sides of the area should be the target an equal number of times. Accordingly, any significant deviation from the expected chance average could be attributed to the PK effect if the throwing for each target were done under uniform conditions. Experimental error due to such factors as bias in the objects thrown, in the manner or direction of release, or in the surface of the area on which they fell could be controlled by the regular alternation of the target areas.

Apparatus

A table approximately 2½ by 3 feet was covered with a thin layer of sponge rubber and surrounded with side walls 6 inches high. Centrally above it at one end a semi-circular rubber-lined chute was mounted at an angle of 45 degrees so that the lower end, raised about 6 inches above the table surface, would discharge the objects rolling down it onto the surface of the table below. At the top of the chute a cylindrical metal container was mounted on two pivots in such a way that its contents could be emptied by a lever into the upper end of the chute. The table below was divided down the middle into A and B sections. At first a white line was used for this purpose, and any objects coming to rest upon it were thrown over again. The necessity of re-throwing was later eliminated by stretching down the center of the table a fine, taut wire which tilted the objects resting on it and thus eliminated doubtful cases.

In order to add variety to the task for the subject, several shapes of objects were used, as follows:

Spheres: 10 white glass marbles
Cubes: 10 white plastic $1\frac{1}{16}$-inch cubes
Coins: 10 twenty-five cent pieces (quarters)

Procedure

The subject stood at the right or left of the chute, as he preferred, but kept his position throughout his trials for both targets. He dropped whichever objects he was using into the container at random and pressed the lever to release them; and as they fell through the chute he "willed" them to fall into the specified area below. The experimenter called the score aloud so that the subject could check on the accuracy of her recording. The subject then

picked up the objects and replaced them in the can. Except for this, he did not touch the apparatus during a trial.

The experimenter sat at the end of the table opposite the chute, where she would not come in contact with the apparatus. She recorded the results in sets of three columns of ten throws each. The target area for the first five throws and the second five throws of each column were alternated so that target area A came first in one column and target area B came first in the next. A different object was thrown for each of the three columns. For example: 10 marbles thrown 5 times for target A, then 5 times for B; then 10 cubes thrown 5 times for target B and 5 times for A; then 10 coins thrown 5 times for target A and 5 for B.

At each session the order of objects and of targets was rotated regularly. If marbles, cubes, coins was the order used in one session it was changed to cubes, coins, marbles for the next one, and if the cubes column began with the A-side one time it was begun with B the next.

One set of three columns, then, consisted of a total of 300 objects thrown. It comprised a unit for a single experimental session except for the last five series in which three sets were completed at a single session.

A total of 15 series were completed. Six of these were done by miscellaneous subjects, each contributing only one or two sets. Nine series. Five of the special-subject series were done by daughters of series. Five of the special-subjects series were done by daughters of the experimenter, two by herself, with daughters recording, the rest by friends. In general it was planned that each series should consist of 24 sets—a total of 7,200 objects; in six series, however, either more or fewer than the planned number were completed depending upon the availability of the subjects.

Results

When all 15 series are pooled, the total of 113,100 objects thrown gave a deviation of +148 which is not statistically significant. The series done by special subjects gave a higher rate of scoring than did those by the miscellaneous subjects, but the difference is not significant. (See Table 1.)

Table 1

GENERAL RESULTS

	Objects Thrown	Runs	Dev.	CR
Misc. Subjects.....	46,800	2,340	− 28	
Special Subjects.....	66,300	3,315	+176	1.37
Total.....	113,100	5,655	+148	0.88

Further subdivision of the data reveals several features of interest in the nine special-subject series, but no interesting internal relationships were found in the miscellaneous series. In this brief summary, then, the special-subject series only will be considered, and it should be kept in mind that the findings are selected and are of value mainly in guiding future research.

When the three-column sets for all special-subject series were pooled, it was found that there was a general tendency for the first column to be near the chance level, the second column to be negative, and the third to be high. The CR for the third column alone is 3.19. (See Table 2.) This tendency toward high scores in the third column is especially marked in the case of the coins, which have a CR of 3.04 in this column.

Table 2

DISTRIBUTION OF SCORES IN THE THREE COLUMNS OF THE SET (SPECIAL-SUBJECT SERIES)

Object	Col. 1		Col. 2		Col. 3		Total Dev.	
	Objects Thrown	Dev.	Objects Thrown	Dev.	Objects Thrown	Dev.	Objects Thrown	Dev.
Marbles.....	7,400	0	7,200	−25	7,500	+ 52	22,100	+ 27
Cubes.....	7,300	+18	7,400	−27	7,400	+ 56	22,100	+ 47
Coins.....	7,400	−23	7,500	− 4	7,200	+129	22,100	+102
Total.....	22,100	− 5	22,100	−56	22,100	+237	66,300	+176
	CR=.07		CR=.753		CR=3.19		CR=1.37	

For a study of the chronological distribution of hits, each special-subject series was divided as nearly as possible into fourths. The first quarters for all series were pooled, as were the second, third,

and fourth. The first chronological period was significantly positive, with a CR of 3.01. (See Table 3.) In this quarter of the data, all three types of objects gave positive results, with the coins scoring highest (CR=2.65). The second and fourth chronological periods tended to be negative while the third quarter was positive. None of the deviations reached the criterion for significance, but when curves are fitted by the sensitive method of orthogonal polynomials it is found that the four totals are adequately and significantly described by the cubic curve (P=.01), suggesting that the alternate rise and fall of the deviations is a real effect.

Table 3

CHRONOLOGICAL DISTRIBUTION OF SCORES FOR MARBLES, CUBES AND COINS (SPECIAL-SUBJECT SERIES)

Chronological Period	MARBLES		CUBES		COINS		TOTALS		CR
	Number Thrown	Dev.	Number Thrown	Dev.	Number Thrown	Dev.	Number Thrown	Dev.	
1st.....	5,700	+36	5,700	+61	5,700	+100	17,100	+197	3.01
2nd.....	5,700	−39	5,700	−30	5,700	− 18	17,100	− 87	1.33
3rd.....	5,300	− 7	5,300	+68	5,300	+ 25	15,900	+ 86	1.36
4th.....	5,400	+37	5,400	−52	5,400	− 5	16,200	− 20	.31
Total.....	22,100	+27	22,100	+47	22,100	+102	66,300	+176	1.37

When an analysis was done to compare the rate of scoring on the three columns in the set through the same four chronological subdivisions of the data, it was found that the third column of the set was the one which contributed most to the high rate of scoring

Table 4

CHRONOLOGICAL DISTRIBUTION OF SCORES FOR THREE COLUMNS IN THE SET (SPECIAL SUBJECT SERIES)

Chronological Period	COL. 1		COL. 2		COL. 3		TOTAL		CR
	Objects Thrown	Dev.	Objects Thrown	Dev.	Objects Thrown	Dev.	Objects Thrown	Dev.	
1st.....	5,700	+23	5,700	+35	5,700	+139	17,100	+197	3.01
2nd.....	5,700	− 9	5,700	−67	5,700	− 11	17,100	− 87	1.33
3rd.....	5,300	+12	5,300	−22	5,300	+ 96	15,900	+ 86	1.36
4th.....	5,400	−31	5,400	− 2	5,400	+ 13	16,200	− 20	.31
Total.....	22,100	− 5	22,100	−56	22,100	+237	66,300	+176	1.37

noted in the first chronological period. (See Table 4.) This column alone has a CR of 3.68. The scoring in the third column of the set also was high during the third chronological period (CR=2.64). In the other column in the set and in the other chronological periods there appear to be no large deviations.

Discussion

It would seem that the effect of the simple target system, which was the main point of the experiment, may have been overshadowed by the complexity of objects used and the variation in types of subjects who took part.

Although several internal features of interest were found in the special-subject series, they cannot be considered statistically significant in view of the fact that they represent only a selected portion of the data. They are of value only insofar as they afford suggestions for further research.

The fact that the special-subject series gave better results than the miscellaneous-subject series leads one to speculate about possible differences in the psychological atmosphere. Since the special-subject and miscellaneous-subject series are not significantly different, however, it would not be justifiable to try to describe the psychological conditions at length.

One main point of difference in the two types of series was probably that of motivation. Eight of the nine special-subject series were done by subjects with considerable experience with both ESP and PK experimental procedures whereas most of the miscellaneous subjects were new to the tests. It is reasonable to suppose that the special subjects, each knowing that he was the sole subject in the series, would take a greater interest in the placement tests and in the objective of the research than would the miscellaneous subjects who were often only casual visitors.

The chronological decline in the special subjects suggests that a 24-set series was too long and that interest, spontaneity, or other psychological factors may change before the end of so large an amount of experimentation. A six-set series would appear to have been sufficient.

It might be of interest to mention that two of the special subjects

each did two separate series. The results for their first series were +112 for 13,800 objects thrown (CR=1.91), and for the second, only +38 for 14,400 objects thrown (CR=.63).

The fact that the last column of the three in a set yielded the highest results suggests the operation of psychological factors too. The low second column results possibly may be the outcome of a degree of relaxation after the first column of throws, the third a re-concentration of energy and motivation after knowledge of previous failure—a sort of "last chance" performance, a psychological terminal effect. This finding would appear to be the reverse of that reported most commonly, the highest score in the first column of the set. However it is possible that the three-attempt nature of the task here, in which the subject is aware of his result after each attempt, may make this case not strictly comparable to others.

The major suggestions from the above experiment are that: (1) special subjects, each of whom does a complete series, produce better results than do miscellaneous subjects, each of whom does only a few sessions of an experiment; (2) fairly short series are better than long ones; (3) the effect of psychological or physical factors in the type of object used for PK placement needs further exploration; and that (4) the effect of set structure needs to be investigated further. The question remains whether or not a three-column set was better than a one-column set would have been.

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A FURTHER STUDY OF THE RELATION OF ATTITUDE TO SUCCESS IN ESP SCORING

By GEORGE W. CASPER

ABSTRACT: The experiment reported here is a further study along the line of work carried out by Schmeidler and Bevan. It was designed to determine the effect on ESP scoring of the subject's attitude toward (1) the possibility that ESP can occur, and (2) the possibility that he himself might have ESP ability. On the basis of their attitudes toward each of these questions the subjects were divided into sheep (those who accepted the possibility), goats (those who rejected it), and indecisives (those who were undecided).

In the two series, 146 subjects carried out two clairvoyance runs and two GESP runs apiece with ESP cards. Their total ESP results were on a chance level. There was a significant decline, however, between the scores of the first and second halves of Series B, a decline which the experimenter attributes to his own change in attitude toward the experiment. When the ESP results were grouped with respect to the subjects' attitudes concerning the possibility that ESP can occur, only one significant difference was found: a difference between the sheep, who scored positively, and the indecisives, who scored negatively.

Mr. Casper is a graduate research assistant in the Parapsychology Laboratory. This is his first article for the Journal.—Ed.

INTRODUCTION

THE EFFECT of a subject's belief about ESP on his level of scoring has already been investigated, chiefly by Schmeidler (**2, 3**) and Bevan (**1**). Dr. Schmeidler divided her subjects into two groups: the "sheep," subjects who accepted the possibility of the existence of ESP; and the "goats," those who did not. She found that the sheep scored above mean chance expectation and that the goats scored below, with the difference in scoring levels being statistically significant. In his experiment, Bevan added a third category, the "indecisives." He found that the indecisive group scored highest, the sheep scored second, and the goats scored at chance level. I designed the experiment reported here chiefly as a further study of the subjects' attitudes toward ESP, although the opportunity was used to collect personality data about the subjects for another study.

PROCEDURE

The experiment was done at Heidelberg College, Tiffin, Ohio, with technical advice given by Mr. J. M. Bevan, Head of the De-

partment of Psychology.[1] The work consisted of Series A, done in the spring of 1950 with 75 subjects, and Series B, done in the fall of 1950 with 71 subjects. The tests were made with standard ESP cards, each subject having one session with two runs of general extrasensory perception (GESP) and two runs of clairvoyance using the BT procedure.

During the session, I, the experimenter, sat in my office and the subject sat two rooms away. Both rooms had the doors closed. The entrances to the two rooms were on different hallways, and so placed that one could not be seen from the other. A signal system was used which consisted of a two-way switch and a small red light in each room. I turned on both lights when a card was in position, and the subject turned off both the lights after recording his call.

The subject, after preliminary instruction, went to his room. I picked one of eight decks at random, shuffled it, and cut it. Then, putting the first card in place, I switched on both lights as a signal to the subject to make his first call. When he had done so he switched out the lights. I then placed the second card in position and switched on the lights, and the test was continued in this manner until a run was completed. After a slight pause, during which another deck was shuffled and cut, the test was resumed for another run. Every subject did two runs with one procedure, followed by two runs with the other, the order of using GESP and BT being alternated from session to session. The subject and I each had his own duplicate record book with numbered sheets, and each did his recording independently. In order to keep up the subject's morale, a checkup of hits was made after each two runs. With these precautions there was no opportunity for sensory cues or motivated errors.

In Series A, each subject was asked in a short interview preceding his ESP test whether he believed in ESP, whether he was undecided, or whether he did not believe in it. On the basis of this interview, he was rated as sheep, indecisive, or goat. Every subject in Series A was asked to fill out the Stuart Interest Inventory and a personality inventory.

[1] The writer wishes to thank Dr. Forrest M. Keen of Heidelberg College for his assistance in obtaining subjects for the experiment.

In Series B it was felt that a written questionnaire would be more objective. One was therefore prepared which would obtain answers to the following questions: (1) Did the subject know what the term ESP meant? (2) Did he believe that ESP was a theoretical possibility? and (3) Did he believe that he himself had ESP ability? This questionnaire was filled out by the subject after doing two runs, but before being told his score. On the basis of his answers to each of these questions, the subject was rated as sheep, indecisive, or goat. In this second series each subject also filled out at the same session the Stuart Interest Inventory, a personality inventory, and made two drawings to be used in determining his expansion-compression rating.

Results

The total ESP results in this experiment are not significant. In Series A, with 300 runs, the total deviation above mean chance expectation was 5 hits. Series B with 284 runs had a total deviation of 25 hits below expectation. There was no difference of any significance between the BT and GESP runs. See Table 1.

Table 1

Total Esp Results

Condition	Series A		Series B		Total	
	Runs	Dev.	Runs	Dev.	Runs	Dev.
GESP	150	+10	142	− 4	292	+ 6
BT	150	− 5	142	−21	292	−26
Total	300	+ 5	284	−25	584	−20
CR	0.1		0.7		0.4	

However, when the scores are grouped according to the attitude of the subjects on the question of the theoretical possibility of ESP, some interesting differences appear. The sheep of both series combined scored 51 hits above chance in 324 runs, whereas the indecisives of both series scored 63 hits below chance in 208 runs, the difference in scoring being significant (CR=2.6). The goats of Series A (no goats were found in Series B) scored just slightly below chance:

52 runs, 8 hits below. With regard to their attitudes about their own personal ESP ability, the subjects in Series B who believed that they had some ability scored 6 hits above chance in 12 runs; those who were undecided about their own ability scored 29 hits below in 200 runs; and those who did not believe they possessed any ESP ability scored 2 hits below in 72 runs.

Table 2

RESULTS AS RELATED TO ATTITUDE TOWARD ESP AS A THEORETICAL POSSIBILITY

Attitude	Series A		Series B		Total			Total CR_d
	Runs	Dev.	Runs	Dev.	Runs	Dev.	CR	
Sheep	128	+43	196	+ 8	324	+51	1.4	2.6
Indecisives	120	−30	88	−33	208	−63	2.2	1.0
Goats	52	− 8	0*		52	− 8	0.6	0.5

*None of the subjects in this series turned out to be goats.

The most significant finding is a marked chronological decline in Series B between the first half of the runs made and the second half. The first 142 runs had a positive deviation of 45 hits, and the second 142 runs had a negative deviation of 70 hits, the CR of the difference being 3.4. No corresponding decline was found in Series A, but the decline within the series remains significant when both series are considered together, as shown in Table 3.

No significant relationship was found in either series between ESP test scores and the Stuart Interest Inventory results. Neither did the expansion-compression ratings upon the subjects' drawings (in Series B) show any significant relation to the ESP card scores.

Table 3

CHRONOLOGICAL DISTRIBUTION OF ESP RESULTS

Chronological halves of series	Series A		Series B			Total		
	Runs	Dev.	Runs	Dev.	CRd	Runs	Dev.	CR_d
1st half series	150	+5	142	+45	3.4	292	+50	2.5
2nd half series	150	0	142	−70		292	−70	

The general personaltiy inventories have not yet been analyzed, since they were collected for another study.

Discussion

Both Schmeidler's experiments and mine may be compared with regard to the subjects' belief in their own ESP ability. Her subjects were designated sheep or goats on the basis of their belief in their own ability to obtain a positive score in the test they were about to take. In my experiment, Question 3 of Series B concerning the subject's attitude toward his own ESP ability resembles Schmeidler's question, although it is not as specific as hers. Since Schmeidler considered undecided persons as sheep, the sheep and indecisives of my experiment will have to be pooled to make a comparison with hers. In the work published to date by Schmeidler (2) the sheep averaged 5.13 hits per run and the goats 4.91. In the limited part (Series B) of this experiment that can be compared with her results, the sheep (sheep and indecisives) averaged 4.89 hits per run and the goats, 4.97. This is a reversal of Schmeidler's results, but it is not statistically significant.

On the matter of the subjects' attitude toward the theoretical possibility that there is ESP, Bevan's results may be compared with those of my experiment. In both these experiments the sheep scored above chance, and the goats scored very near the chance level. The indecisives of the two experiments, however, show a marked difference. Bevan's indecisives were significantly positive and scored higher than either his sheep or goats. On the other hand, my indecisives scored lower than either my sheep or goats. In general those of my subjects who were classed as indecisives appeared to me to have an indifferent attitude which may have been a factor in their making a negative score of marginal significance. At any rate, the indecisives in both experiments showed the most evidence of ESP, although their scores deviated in opposite directions from mean chance expectation.

The fact that chance results were obtained on the Interest Inventory and the expansion-compression breakdowns suggests that there are subtle psychological factors necessary to the successful operation of these tests which have not yet been discovered. The

interest test and the expansion-compression breakdown have been found to fail in some specific groups before, but the reasons for that failure are still unknown.

Statistically the most significant finding in the experiment is the decline in Series B. The 284 runs of the series took about 20 days to complete, with the testing distributed fairly evenly over that period. When the first 142 runs are compared with the last 142 runs, a decline is found which gives a CR of 3.4. Since each subject did his four runs at one time, there was no possibility that loss of interest on the part of the subjects could have been a factor in the decline. Also, the subjects were unselected; all the students in the psychology and sociology classes were given an opportunity to participate at whatever time suited them best. Nor was there any noticeable change in campus life to account for the decline. After considering these and many other possibilities, it appears that there is only one explanation for the decline—a change in my own attitude which somehow affected the subjects.

The facts in the matter support this view. I was on a short leave from Duke University to do the experiment at Heidelberg College. During the first week or so, the work went slowly because there were not many subjects. I was able to be free and easy with each subject and to take as much time as necessary for the runs. However, as time passed, it became apparent to me that the experiment was not proceeding rapidly enough for me to get back to my classes at Duke at the beginning of the semester there. I therefore made the schedule sheet more readily available to get more subjects, and I speeded up the testing process as much as possible. This meant that I was tired at the end of a day and was not as friendly and sociable with the subjects as I had been previously. And it also meant that the subjects were hurried through the testing procedure one after another because I was anxious to finish the experiment and return to Duke. In the light of the facts, this experiment would seem to indicate again what ESP workers have emphasized so much, that the attitude of the experimenter is a prime factor in getting good results.

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LETTERS AND COMMENTS

AN APPLICATION OF FIELD THEORY TO ESP

To the Editors:

One of the more puzzling findings in ESP is that only certain people can score above chance and that they can do so for only limited periods. It is also puzzling that those few subjects who have scored perfect or near perfect runs should completely lose this special knack and return to chance. One explanation which these conditions suggest is that there is more going on here than a single response to a single stimulus.

Sherif, in his *Psychology of Social Norms,* points out that individuals in an ambiguous situation relate their behavior to some sort of framework. This framework may be supplied by other individuals who comprise a group, or if the individual is isolated, he builds up his own framework in terms of his own behavior. In the ESP situation there seem to be at least two factors, in addition to the stimulus card, to which guesses might be related. These factors are the previous guesses which a subject used on that run, and the guesses which he is saving for later use. Previous guesses might operate in terms of each single preceding guess or in terms of a sequence established by a series of preceding guesses. Sherif's findings would suggest that a series of previous guesses are exerting an influence.

According to this concept one might say that a subject has ESP ability, but the persuasive influence of the other factors in his perceptual field misguides him. If this condition is true, one might be able to explain the wrong guesses of a good subject in terms of logical transformations of the stimulus by these other field factors.

Sincerely yours,

Richard Myrick

GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference.

A simple description of the basic experimental methods and of the statistical procedures to be followed in evaluating ESP (both card tests and drawings tests) and PK may be found in the *Handbook of Tests in Parapsychology* by Betty M. Humphrey (published by the Parapsychology Laboratory, Duke University, Durham, N. C.).

AGENT: In tests for telepathy, the person whose mental states are to be apprehended by the percipient. In GESP tests, the person who looks at the stimulus object.

AVERAGE SCORE: Average number of hits per run.

BT ("Broken Technique"): The clairvoyance technique in which each card is laid aside by the experimenter as it is called by the subject. The check-up is made at the end of the run.

CALL: The ESP symbol selected by the subject in trying to identify a target.

CHANCE: The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely score if only chance obtains.

CHANCE AVERAGE: Mean chance expectation expressed as an expected score, generally in terms of average per run.

CHI SQUARE: A sum of quantities each of which is a deviation squared divided by an expected value. Also a sum of the squares of CR's.

CLAIRVOYANCE: Extrasensory perception of objective events as distinguished from telepathic perception of the mental state of another person.

CR (CRITICAL RATIO): A measure to determine whether or not the observed deviation is significantly greater than the expected random fluctuation about the average. The CR is obtained by dividing the observed deviation by the standard deviation. (The probability of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the average scores of two samples of data divided by the standard deviation of the difference.

DECK: Twenty-five ESP cards.

CLOSED DECK: An ESP deck composed of five each of the five symbols.

OPEN DECK: An ESP deck made up of the ESP symbols selected in random order.

DEVIATION: The amount an observed number of hits or an average score varies from the mean chance expectation or chance average. A deviation may be total (for a series of runs) or average (per run).

DIE THROW: The elementary unit of data in a dice-throwing test represented by the throwing and reading of a single die, no matter how many dice are thrown at a time.

DISPLACEMENT: ESP responses to targets other than those for which the calls were intended.

BACKWARD DISPLACEMENT: ESP responses to targets preceding the assigned targets (the ones for which they were intended). Displacement to the targets

one, two, three, etc. places preceding the assigned target are designated as (—1), (—2), (—3), etc.

FORWARD DISPLACEMENT: ESP responses to targets coming later than the assigned targets. Displacement to the targets one, two, three, etc. places after the assigned target are designated as (+1), (+2), (+3), etc.

DT (Down Through): The clairvoyance technique in which the cards are called down through the deck before any are removed or checked.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibility of the hypothesis being tested.

ESP (EXTRASENSORY PERCEPTION): Response to an external event not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol. 1, March, 1937.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to chance alone.

FREE MATERIAL: The stimulus objects of experiments in which an unlimited or unspecified range of stimulus objects is employed (as contrasted with methods such as card-calling in which the subject knows that the stimulus object is one of a known range).

FREE RESPONSE METHOD: An ESP test method in which a relatively unlimited range of stimulus objects is used and in which the subject is instructed to express freely as his response any range of thoughts and ideas.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of extrasensory perception, permitting either telepathy or clairvoyance or both to operate.

HIGH-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling eight or more.

LOW-DICE TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling six or less.

MEAN CHANCE EXPECTATION; see CHANCE.

P (PROBABILITY): A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with those psychical effects which appear not to fall within the scope of what is at present recognized law.

PERCIPIENT: The person who makes the calls in an ESP test.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a subject without any known intermediate physical energy or instrumentation.

PRECOGNITION: Cognition of a future event which could not be known through rational inference.

PREFERENTIAL MATCHING: A method of scoring free responses. A judge ranks the stimulus objects (usually in sets of four) with respect to their similarity to, or association with, each response; and/or he ranks the responses with respect to their similarity to, or association with, each stimulus object.

PSI: A general term to identify personal factors or processes in nature which transcend accepted laws. It approximates the popular use of the word "psychic" and the technical one, "parapsychical."

PSI PHENOMENA: Occurrences which result from the operation of psi. They include the phenomena of both ESP (including precognition) and PK.

QD (QUARTER DISTRIBUTION): The distribution of hits in the record page (or in a logical subdivision thereof, such as the set or the half-set) as found in the four equal quarters formed by dividing the selected unit horizontally and vertically.

RUN: A group of trials, usually the successive calling of a deck of 25 ESP cards or symbols. In PK tests, 24 single die throws regardless of the number of dice thrown at the same time.

SCORE: The number of hits made in one run.

TOTAL SCORE: Total of scores made in a given number of runs.

AVERAGE SCORE: Total score divided by number of runs.

SD (STANDARD DEVIATION): Usually the theoretical root mean square of the deviations. It is obtained from the formula $\sqrt{npq}$, in which n is the number of single trials, p the probability of success per trial, and q the probability of failure. (For ESP cards, SD $= 2\sqrt{}$no. of runs.) Sometimes the SD used is that estimated from the observed variability in the scores.

SD OF THE DIFFERENCE: For both ESP cards and PK tests using dice, the SD of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the SD of a single run and R_1 and R_2 are the number of the runs in the respective samples compared. This gives the SD of the difference for run score averages.

SERIES: Several runs or experimental sessions that are grouped in accordance with a stated principle.

SESSION: A unit of an ESP or PK experiment comprising all the trials of one test occasion.

SET: A subdivision of the record page serving as a scoring unit for a consecutive group of trials, usually for the same target.

SEVENS TEST: A PK technique in which the aim of the subject is to try to influence a pair of dice to fall with the two upper faces totaling seven.

SIGNIFICANCE: A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. The criterion commonly used in this JOURNAL is a probability value of .02 or less, or a deviation in either direction such that the CR is 2.33 or greater.

SINGLES TEST: A PK technique in which the aim of the subject is to try to influence dice to fall with a specified face up.

STIMULUS OBJECT: The ESP card or drawing or other object, some identifying characteristic of which is to be apprehended by the percipient.

TARGET: In ESP tests, the stimulus object; or in telepathy, the mental state of the agent. In PK tests, the faces of the die (or combination of faces) which the subject attempts to bring up in the act of throwing.

TARGET CARD: The card which the percipient is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

SUBJECT: The person who is experimented upon. In ESP tests, most commonly the percipient (though also the agent in GESP and telepathy). In PK tests, any individual whose task it is to influence the objects thrown.

TARGET DECK: The deck of cards the order of which the subject is attempting to identify.

TARGET FACE: The face on the die which the subject tries to turn up as a consequence of direct mental action.

TELEPATHY: Extrasensory perception of the mental activities of another person. It does not include the clairvoyant perception of objective events.

TRIAL: In ESP tests, a single attempt to identify a stimulus object. In PK tests, a single throw of the dice or other objects thrown.

WHEN A MAN is conscious, one may conceive that within his brain impulses are passing along a million insulated nerve fibres . . . impulses that are somehow co-ordinated into the orderly sequences of deliberate thought.

What is the real relationship of this mechanism to the mind? Can we visualize a spiritual element of different essence capable of controlling this mechanism? When a patient is asked about the movement which he carries out as the result of cortical stimulation, he never is in any doubt about it. He knows he did not will the action. He knows there is a difference between automatic action and voluntary action. He would agree that something else finds its dwelling-place between the sensory complex and the motor mechanism, that there is a switchboard operator as well as a switchboard.

—Wilder Penfield, "The Cerebral Cortex and the Mind of Man" in *The Physical Basis of Mind.* (Oxford: Blackwell, 1950).

THE PROBLEM OF BODY-MIND RELATIONSHIP

I SUPPOSE everyone who tries to understand what is going on in the brain must suffer from time to time from the feeling that he has chosen a really insoluble problem, not merely a very difficult one. Heaven knows, it is going to be difficult enough to make an intelligible picture of the working of an organ which has millions and millions of parts and is responsible for all the complicated things we can do; but difficulties of that sort could be faced. The real trouble comes from the feeling that there may be an important part of the picture which can never be fitted in, however long we may work at it.

Sherrington explained why we should have these misgivings. The part of our picture of the brain which may always be missing is, of course, the part which deals with the mind, the part which ought to explain how a particular pattern of nerve impulses can produce an idea; or the other way round, how a thought can decide which nerve cells are to come into action. It is a very old difficulty and nowadays some of the philosophers tell us that if we look at it in the right way it is not as serious as it seems. But the physiologist cannot look at it except as a natural scientist and at present that seems almost certain to lead him into trouble. My own feeling is that before the trouble comes to a head it will have been solved by some enlargement of the boundaries of natural science, by the progress of psychology, for instance. In fact, psychology can scarcely get along without coming to terms with the relation of body and mind.

—E. D. Adrian, "What Happens When We Think,"
in *The Physical Basis of Mind.*
(Oxford: Blackwell, 1950).